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HEREDITY AND EVOLUTION

HEREDITY AND EVOLUTION

By

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PREFACE

THE rediscovery of Mendel's laws in 1900 was an important event for biological science, which had been handicapped during the latter part of the nineteenth century by lack of knowledge about heredity. The theory of evolution was especially influenced, though for some years genetical workers were faced with the difficulty that the Mendelian scheme did not easily account for one of the most conspicuous features of living things : the existence of separate species, often sharply distinguished from one another.

These difficulties are now being removed, largely as a result of the discovery that the behaviour of the hereditary units can be studied under the microscope ; and it seems probable that all, or nearly all, the various differences between one form of life and another will be related to a few simple principles. In some important ways the problem is by no means solved, but the attitude of genetics towards evolution can be clearly stated.

In this book, the principles of genetical science, their scope and the evidence on which they rest have been discussed ; the bearing of these principles on the theory of evolution being kept in view throughout. Illustrations likely to be familiar to those with little biological knowledge have been given whenever possible.

For their kindness in reading the manuscript I am indebted to Professor R. C. Punnett, Gonville and Caius College, Cambridge ; Mr. C. H. Waddington, Christ's

College, Cambridge; Mr. K. B. Smellie, London School of Economics and Political Science; and Mr. J. W. Stork, Charterhouse School. And to the following for permission kindly given to reproduce or copy figures: Messrs. J. and A. Churchill (PLATE 1), Mr. E. B. Ford and Messrs. Methuen (PLATE 2), Professor R. C. Punnett and Messrs. Macmillan & Co., Ltd. (fig. 1), Mr. G. U. Yule and Messrs. Charles Griffin & Co., Ltd. (figs. 3 and 4), the editors of *Genetics* (fig. 6), and Dr. Lillian Hollingshead (fig. 24).

A. E. WATKINS.

Cambridge, July, 1935.

CHAPTER I

INTRODUCTION

THE ORIGIN OF SPECIES, by Charles Darwin, was first published in 1859. It was there suggested that all living organisms, plant or animal, had evolved from simpler forms of life by a gradual process that might be described as descent with modification. During the remainder of the nineteenth century, led by Darwin and Wallace, the chief preoccupation of biologists was to provide evidence for this theory, to try to trace lines of descent, and to pursue other inquiries directly suggested by the new idea. Today, through these labours, the greater part of biological science rests, implicitly if not explicitly, on the evolutionary principle; and no biologist is likely to deny its general truth. At the very least it may be confidently foretold that any theory that replaces it will have to be closely similar in most of its implications.

Outside the world of biology the idea of evolution has considerably enlarged man's outlook, and nowadays is rarely questioned; its ready acceptance being assisted, no doubt, by the fact that it is well illustrated by the growth of human institutions, and in other familiar fields. From geology, for example, it is well known how the present configuration of the earth's surface, and the arrangement of its strata, have come about gradually, after long periods of time, through agencies such as the denuding effects of glaciers and streams that can be seen at work today.

In earlier times it was accepted without question that all the forms of life were fixed, and had remained unchanged from the time of their creation; but more liberal views had actually been expressed some time before Darwin. Thus in 1801, Lamarck had supposed that in certain cases species had arisen from other species by a process of gradual modification due to the effects of use and disuse, and especially of conscious effort. A man strengthens his arms by use; and this added strength, it was supposed, would tend to be transmitted to his offspring. Snakes acquired their slender forms, and lost their limbs, by making constant efforts to pass through narrow crevices.

The credit for the idea of organic evolution is rightly given to Darwin, however, since he boldly applied the theory to explain the origin of all forms of life from one ancestral form; brought forward a wealth of evidence and argument to support his thesis; and suggested how the process might be brought about. The case was most convincingly presented. Many facts and arguments were adduced to show that species, far from being fixed and changeless, were always altering. Special stress was laid on the variability of domesticated animals and plants, and on the changes wrought in these by man's conscious selection, subjects later dealt with exhaustively in a separate work, published in 1868. He pointed out that variation, differences between one member of a species and another, could as easily be found in natural species as in domesticated; so that, if selection occurred, there also change would follow. For this natural selection, as it is termed, he suggested that an agent might be found in the strong competition that exists in Nature—competition now familiar to any student of natural history—because the numbers born into the world exceed so greatly those that can continue to exist. Only those best fitted to their

environment would survive. These, he argued, would transmit their qualities to their offspring, which would therefore differ from the average of the previous generation by their better adaptation. Finally, this change itself produces different conditions for other organisms, so that change would be perpetual : the successful stoat reduces the rabbit population, thus affecting the vegetation, and through it many other animals.

This theory contains much truth, and it passed practically unquestioned for forty years. Since 1894, however, when Bateson reopened the matter, it has been increasingly clear that in some important respects the mechanism proposed rested on unproved assumptions, or was inadequate ; and here very considerable advances have been made since Darwin's day, especially in the present century. Details have been filled in, and much that was obscure made plain. The points at issue will be most easily understood by considering three different theories that have been advanced at one time or another to explain how living organisms might change.

The first is Lamarck's, now generally abandoned. On his view the giraffe might be descended from an animal with a short neck, and would owe his present form to the constant efforts of his ancestors to reach upwards, when grazing from the leaves of trees. By continued effort the individual increased the length of its neck, and some of this increase was transmitted to its offspring. After many generations a long-necked animal was the result.

It will be noticed that Lamarck had not realized the principle of natural selection at all. He suggested a cause for the origin of new variations, forms unlike the old. Darwin, on the other hand, who was responsible for the second theory, argued that the fact of variation, the fact that the individual members of a species are not all alike, must be

universally admitted. He accepted the existence of some degree of variation, and did not explain it. From this fact, and the existence of heredity, which was also generally accepted, he considered that evolution must necessarily follow on account of the principle of natural selection. His theory did not take account of the nature either of variation or of heredity. The account he would give of the origin of the giraffe from short-necked ancestors would differ essentially from Lamarck's. He would say that the reasons for variation were not well known, but it would be a fact that some of the short-necked ancestors were born with longer necks than others. These would be able to reach foliage beyond the reach of their fellows, and in some districts this would give them a definite advantage in the keen struggle for existence. Those with the longest necks would survive, and would transmit the advantage to their offspring; the process would continue and animals with longer and longer necks would be produced. Evolution was the result of the intensity of the competition, which would be so great—as, with considerable justice, he argued—that any advantage, no matter how small, would tend to be selected.

The third view is to be found elaborated with a wealth of illustration in *The Mutation Theory*, by the Dutch botanist de Vries, published in 1900, but foreshadowed in some of its essentials by Bateson six years earlier. It lays more stress on the character of the variations than on natural selection and definitely supposes that effective variations are discontinuous, arising suddenly instead of by slow degrees. Examples of these mutations, or sports as they are often called are well known in domesticated animals and plants—Darwin, indeed, gave many instances, but to him they were just one kind of variation out of many, instead of the only effective kind. In the case of the giraffe it would be supposed

that, among the short-necked ancestors, one or more individuals with markedly longer necks appeared suddenly by chance, and were able to multiply their kind because a plentiful food supply was available to them. From these, the giraffe was eventually descended by a series of mutations for longer necks. On this view the direction of evolution will be decided by the variations that arise. Natural selection merely weeds out those that are ill adapted to their environment, but cannot otherwise determine the direction of the changes. What determines the mutation is still to be discovered.

The differences between these three theories may be illustrated by supposing that we wanted to breed a race of human giants twelve feet high. According to Lamarck this would be gradually brought about by constantly encouraging, for generation after generation, all pursuits that tended to increase the height. According to Darwin we should simply select the tallest members of each generation for parents, and continue the selection until the object was attained. On the mutation theory, neither method would be any use ; and we should have to wait for taller individuals to arise spontaneously—unless we could find out how to induce mutations.

Are any of these theories correct? An answer has been given by the modern science of genetics—the study of heredity and variation—which, during the last thirty years, has filled in many of the gaps left by Darwin, and in places contradicted his conclusions. It may be stated at once that the mutation theory came most nearly to the truth ; but that the amount of importance to attach to natural selection is still difficult to assess. Lamarck's theory may be dismissed. Attempts to prove the phenomenon he postulated —the inheritance of acquired characters as it is called—have been frequent ; but have always led to a negative result.

His supposition is also difficult to reconcile with our positive knowledge on the subject of heredity. For these two reasons it may be safely disregarded.

Heredity has long been accepted as a familiar, but mysterious, fact. It could not be expected that much would be learned about it until the discovery of the microscope, since the essential elements in sexual reproduction—the two cells, derived from the parents, which unite to form the embryo—are microscopic in size. It was not until the end of the seventeenth century that it was found that the fertilizing fluid from male animals contained innumerable minute bodies which, having the power of movement, were called spermatozoa. The discovery led to a bitter controversy concerning the origin of the embryo. It was believed at the time that the embryo was a complete replica, in miniature, of the adult organism, which arose from it by a process of unfolding ; and the discovery of spermatozoa divided biologists into two schools, those who believed that the embryo was encased in the sperm and those who maintained that it was to be found in the egg-cell of the female. It was not until the nineteenth century that the middle way was discovered and it was shown that the embryo came from the fusion of the two cells, egg-cell and sperm ; and that subsequent development was the result of repeated divisions of the zygote, the product of fusion.

This was the state of knowledge in Darwin's day. The process of fertilization was not clearly understood ; though the underlying principle had been grasped sufficiently well, for example, for Mendel, who had been breeding sweet peas from 1853 to 1866, to interpret his results quite accurately.

It will now be understood that heredity can be studied in two ways. First, by the direct method. Having recorded the characters of two selected parents, the occurrence

of these characters in the offspring can be observed, and from these observations the mechanism of transmission can be deduced. This was the method of Mendel, and the essential features of his discovery were twofold. In the first place he showed that heredity consisted in the transmission of constant units from one generation to the next. Secondly, that although the ordinary individual contains a double set of units, one obtained from the mother and the other from the father, it only transmits a single set of these to its offspring. The process makes it possible for one generation to differ somewhat from the next; but, since the units transmitted are themselves constant, the possible amount of change is limited. These conclusions have important consequences for evolution, and Mendel's discovery must be dealt with in considerable detail.

Alternatively, heredity could be studied indirectly, by observing the physical process of transmission from parent to offspring, and finding out how the material transmitted gives the new organism. This involves the study of cells, the units of structure of all forms of plant or animal life. Both egg-cell and spermatozoon, or male gamete, are single cells; and the product of their union gives a new organism by a series of cell divisions. So that, to understand heredity properly, inquiry must eventually be widened to include cytology—the study of cells and cell division.

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CHAPTER II

MENDELISM

THE problem of heredity had attracted attention long before Mendel's day, and some important observations had been made. The first inquiry to be recorded was carried out in Germany from 1760 onwards by Kölreuter, who made a large number of crosses between different species and varieties of plants. By his experiments he discovered a great deal about the way in which the pollination of plants is brought about in nature; and made it finally certain that the seeds were produced by a sexual process similar to that prevailing in animals; since, when two different forms were crossed the hybrid partook of the characters of both parents. He also found that although the hybrids might be intermediate, or might resemble one parent more closely than the other, it made no difference which of the two had been the female parent and which the male—the result was the same in either case. Another phenomenon he noticed—one that has attracted interest in modern times but is still not properly explained—was that very often the hybrid grew more vigorously than either parent. Sometimes he was able to raise a further generation from his hybrids, but although he accumulated a useful mass of information the essential nature of the hereditary process escaped him.

Towards the end of the eighteenth century Thomas Andrew Knight raised a large number of hybrids, parti-

cularly from fruit trees, in the attempt to produce improved varieties. The method had of course long been applied by animal breeders to improve existing varieties or to establish new ones, but Knight was the first to apply it to plants, and he met with considerable success. Though he made some interesting observations, however, he also failed to solve the problem.

The work of other hybridizers will be passed over. Like Kölreuter and Knight, they explored the field but did no more. Complete success was first attained by Gregor Mendel, abbot of Brünn in Austria, who carried out his experiments on the field pea, *Pisum sativum*, between the years 1853 and 1868. He published his results in 1866, but his contemporaries failed to appreciate their value and they lay neglected until 1900 when Correns, Tschermak, and de Vries independently rediscovered his paper. At that date its value was immediately realized.

Mendel's success was due to his keen powers of observation and analysis, and to the accuracy of his experiments. He selected the field pea for his researches because it seemed to him to fit very well what he considered to be the needs of the study. In the first place it was a species that was to be found in a number of distinct forms that were constant and easily recognizable. Secondly, the hybrids between these forms were fully fertile and it was possible to observe all their progeny through successive generations. Finally, there was practically no danger that error would arise through the application [redacted], since the anthers burst within the bud [redacted] stigma becomes covered with pollen before the flower opens, so that self-fertilization is almost certain. In other words, the parentage of each seed was known with certainty.

At the beginning of his experiment Mendel selected some easily distinguishable varieties and grew them for two years

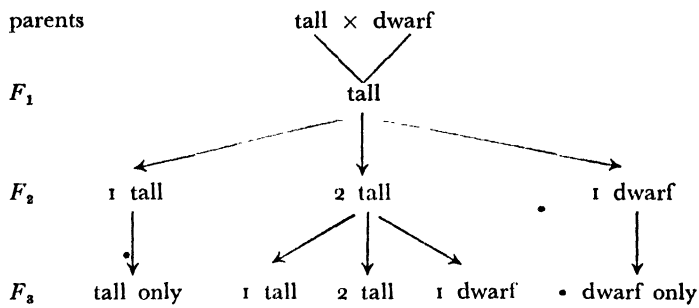
that is, for two generations. They bred perfectly true so far as he was able to observe, and indeed continued to do so throughout the experiments.

He then crossed different types and followed the inheritance of seven well-contrasted pairs of characters. The crosses were made reciprocally, that is, two varieties were crossed together first with one and then with the other as female parent. He found that reciprocal crosses not only looked alike, as Kölreuter had observed, but also bred exactly the same.

No attempt was made to follow differences between varieties unless they were clear cut; all the characters whose inheritance he wished to follow being specially chosen because the different forms could be classified for them quite easily. Such a pair of characters was an extreme difference in height, one variety being from six to seven feet high and the other from nine to eighteen inches only. The hybrid between these two varieties—the F_1 or first filial generation as it is called—was as tall as the tall parent; it might even be slightly taller, a fact that Mendel rightly attributed to the greater luxuriance in growth so often found in hybrids. The character tall was therefore called the *dominant*, and dwarf the *recessive*.

The F_1 plants were allowed to self-fertilize and the seeds they produced gave a further generation, the F_2. This was found to consist of 787 plants that were tall, like the tall parent, and 277 dwarf, like the dwarf parent. No intermediate types were found. Taking this ratio, 787 : 277, with similar ratios he found for other pairs of characters, he concluded that it was an approximation to 3 : 1. The total figures obtained from a number of experiments gave a ratio of 2·98 : 1. He then raised a further generation, the F_3, by self-fertilizing the plants of the F_2, and found that all the dwarf plants bred true; but of the talls only one-third bred true, the remaining two-thirds splitting up once

more into tall and dwarf in the ratio 3 : 1. This means that the F_2, though it appears to consist of only two classes of plants in the proportion 3 tall : 1 dwarf, really consists of three classes in the proportion 1 tall breeding true to tall : 2 tall splitting up into tall and dwarf : 1 dwarf breeding true to dwarf.



To explain these results Mendel supposed that the tall parent carries something, *T*, which produces tallness ; and the dwarf plant something, *t*, which produces dwarfness. *T* and *t* were called *factors* for tall and dwarf. Crossing the two forms gives an F_1, *Tt*, which contains *T* derived from one parent and *t* derived from the other. But although the F_1 carries both factors, or *genes*, as they are often called today, it resembles the tall parent because *T* is dominant to *t*. He further assumed that when the F_1 produces germ-cells either *T* or *t* is included in each one. The two factors are never transmitted both to the same germ-cell. Further, it was reasonable to suppose that *T* and *t* were present in the germ-cells in equal proportions, half containing one and half the other. The F_2 would then be produced by random mating between male gametes which are *T* or *t* and egg-cells which are also either *T* or *t*. The composition of the F_2 would therefore be given as follows :

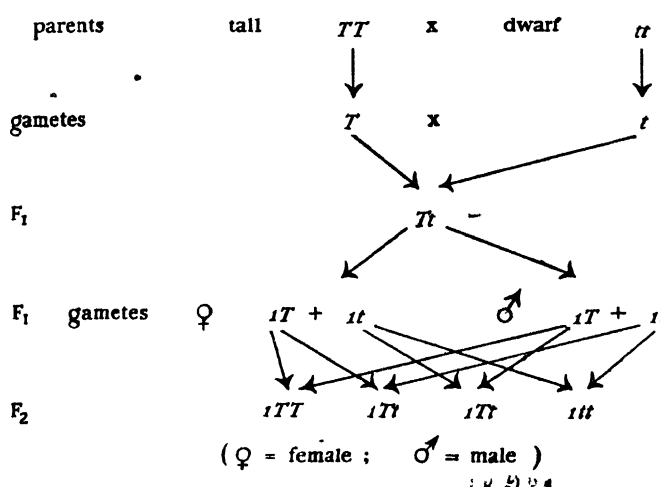
T	egg-cell	× T	male	gamete	gives	TT
T	„	× t	„	„	„	Tt
t	„	× T	„	„	„	Tt
t	„	× t	„	„	„	tt

Total $1TT + 2Tt + 1tt$

The result is obviously given simply by multiplying $1T + 1$ by $1T + 1t$, that is to say:

$$F_2 = (1T + 1t) \times (1T + 1t) = (1T + 1t)^2 = 1TT + 2Tt + 1tt$$

The TT and tt plants could only transmit T or t respectively to their offspring and would breed true; but the Tt plants would split up again into tall and dwarf like the F_1. The whole process may be represented by the following diagram:



It is fundamentally important to genetical theory that the talls and dwarfs extracted in F_2 are exactly like the two parent forms in height. This means that T and t are entirely unaffected by their association in the zygote, and separate uncontaminated. They behave as constant units. The

whole of genetical science is based upon this fact, which has been confirmed by innumerable investigations, often continued for many generations.

The process by which *T* and *t* separate from one another when the germ-cells are formed, and pass unchanged to separate cells, is called *segregation*. Two factors such as *T* and *t*, which segregate from one another in this manner, are said to form a pair of *allelomorphs*. An organism which, like *TT* or *tt*, contains two like factors is said to be a *homozygote*, or to be *homozygous* for that factor. A homozygous individual will breed true. An organism, like *Tt*, which contains both members of an allelomorphic pair, is said to be a *heterozygote*, or to be *heterozygous* for either of the factors in question. Heterozygotes do not breed true.

It will be realized that the ratio 3 : 1 is only an average result to be expected if a sufficiently large F_2 is examined. If a coin is tossed a very large number of times we expect heads to be as frequent as tails; that is to say, heads and tails should be found in the ratio 1 : 1. But if it is tossed only a small number of times, say six or even less, the ratio found might depart widely from the expected equality. In the experiment of Mendel's we have just described, the numbers actually found were 787 : 277, or 2·84 : 1. In other experiments numbers such as 9 : 3, 10 : 5, 607 : 197, 15 : 3 have been found. Some larger figures obtained by Mendel were 14,949 : 5,010, or 2·98 : 1.

In all the seven cases of inheritance worked out by Mendel in the pea, one member of a character pair was dominant to the other. But dominance, though quite common, is not a necessary phenomenon. It happens almost as often that the F_1, and the heterozygotes of later generations, are intermediate between the two parents. The "blue Andalusian" fowl, described by Bateson and Punnett, is a good example. These birds are heterozygotes, *Bb*, so that, despite

all their efforts, poultry fanciers were never able to obtain a true breeding blue. When mated together they give in addition to blues the two homozygous forms: black, and white with a few black splashes, both of which breed true as we should expect. Splashed white, *bb*, mated to black, *BB*, gives blue again.

Blue Andalusians $Bb \times Bb$

$\downarrow$

$1BB + 2Bb + 1bb$

(black) (blue) (splashed white)

In breeding work cases of this kind are an advantage, since each of the three genetically different types that occur in F_2 can be recognized at once by its appearance, and it is possible to pick out any individual and to say at once how it will breed. When dominance occurs we know that the recessive form will breed true ; but we cannot tell except by trial whether the other individuals are homozygous dominants that will breed true, or heterozygotes that will split up once more.

When a heterozygote such as the F_1, is crossed back to either parent it gives two genetically different types in a 1 : 1 ratio. *Bb* for example gives egg-cells and male gametes in the ratio $1B : 1b$; crossed to *BB*, therefore, it will give the two zygotes *BB* and *Bb* in the ratio 1 : 1, and crossed to *bb* it will give $1Bb : 1bb$. That is to say, the blue Andalusian crossed to black gives 1 black : 1 blue ; crossed to splashed white it gives 1 blue : 1 white.

	Blue *Bb*
	$\downarrow$
gametes	$1B : 1b$
crossed by black, *BB*, gives	$1BB : 1Bb$ (black) (blue)
crossed by white, *bb*, gives	$1Bb : 1bb$ (blue) (white)

Besides following the inheritance of various characters by taking each pair separately, Mendel made crosses between forms that differed in two or more ways so as to determine whether these differences were inherited independently of one another or not.

A good example is given by some characters of the seed. These may differ in colour, some varieties having green seeds and others yellow, and also in shape, the seeds of some varieties being wrinkled and of others round. It was found that these two pairs of characters were inherited independently. The total ratio of round to wrinkled, and of yellow to green, was 3 : 1 ; and there was no sign of any association between the members of different pairs, such as all round seeds being also yellow. Instead, the ratio of round to wrinkled was the same among the green seeds as among the yellow, the actual numbers observed being 315 round yellow seeds : 101 wrinkled yellow : 109 round green : 32 wrinkled green, a ratio for the four types of approximately 9 : 3 : 3 : 1.

This ratio arises as a consequence of the fact that the two pairs of factors segregate independently of one another. No further principle is involved ; but when more than one factor is concerned the composition of the F_2 is rather more complicated to work out. The two parents, round yellow and wrinkled green, had the formulae *RR YY* and *rr yy*, and the F_1 the formula *Rr Yy*. When segregation occurs, so far as *R* and *r* alone are concerned the gametes are produced in the ratio 1*R* : 1*r*, and so far as *Y* and *y* are concerned in the ratio 1*Y* : 1*y* ; and so long as *R* and *Y* are independent of one another the gametes produced will consist of all four possible combinations *RY*, *Ry*, *rY* and *ry*, in equal proportions, namely 1*RY* : 1*Ry* : 1*rY* : 1*ry*. Algebraically, the F_2 from this cross is simply obtained by multiplying the egg-cell and male gamete populations together, giving :

$$(1R\Upsilon + 1Ry + 1r\Upsilon + 1ry)(1R\Upsilon + 1Ry + 1r\Upsilon + 1ry) = (1R\Upsilon + 1Ry + 1r\Upsilon + 1ry)^2$$

$$\text{or } (1R + 1r)^2(1\Upsilon + 1y)^2$$

Alternatively, it may be worked out without using algebra. An egg-cell $R\Upsilon$ has an equal chance of being fertilized by any of the four sperms $R\Upsilon$, Ry, $r\Upsilon$, ry, and will give the four zygotes $RR\Upsilon\Upsilon$, $RR\Upsilon y$, $Rr\Upsilon\Upsilon$, $Rr\Upsilon y$, in equal proportions. The other egg-cells, Ry, $r\Upsilon$, ry, may be fertilized by male gametes of the same four kinds. The F_2 can therefore be found by writing the series of four egg-cells four times and supposing that the first series are fertilized by $R\Upsilon$, the next by Ry and so on. This is often called the chessboard method.

$R\Upsilon$ $R\Upsilon$ round yellow	Ry $R\Upsilon$ round yellow	$r\Upsilon$ $R\Upsilon$ round yellow	ry $R\Upsilon$ round yellow
$R\Upsilon$ Ry round yellow	Ry Ry round green	$r\Upsilon$ Ry round yellow	ry Ry round green
$R\Upsilon$ $r\Upsilon$ round yellow	Ry $r\Upsilon$ round yellow	$r\Upsilon$ $r\Upsilon$ wrinkled yellow	ry $r\Upsilon$ wrinkled yellow
$R\Upsilon$ ry round yellow	Ry ry round green	$r\Upsilon$ ry wrinkled yellow	ry ry wrinkled green

Counting up the squares gives the ratio 9 : 3 : 3 : 1 that was actually found.

If the F_1 were back-crossed to the recessive parent we

should obviously obtain the four types of seed (round yellow, round green, wrinkled yellow, and wrinkled green) in equal proportions.

$$(1R\Upsilon + 1Ry + 1r\Upsilon + 1ry) \times ry = 1Rr\Upsilon y + 1Ryy + 1rr\Upsilon y + 1rryy$$

Or, in more detail:

$$RR\Upsilon\Upsilon \times rryy$$

$$\downarrow$$

$$F_1 \qquad Rr\Upsilon y$$

F_1 gametes $1R\Upsilon + 1Ry + 1r\Upsilon + 1ry$

$F_1 \times ry$ $\quad 1Rr\Upsilon y + 1Ryy + 1rr\Upsilon y + 1rryy$

The important conclusion that follows from the cross with two pairs of factors is that the different pairs are independent; so that starting with two types, one with round green and the other with wrinkled yellow seeds, hybridizing gives in the second generation two new forms with round yellow and wrinkled green seeds respectively. This recombination of characters, as it is called, is the principle that underlies the practice of plant breeding by hybridization.

Hybridization as a method of plant improvement had been practised in England by Thomas Andrew Knight as long ago as the end of the eighteenth century, and achieved many successes; but lack of exact knowledge about heredity greatly increased the difficulties of the method. Upon the rediscovery of Mendelism it was pointed out by Biffen at Cambridge, and in Sweden by Nilsson-Ehle, that the principle of segregation and recombination should make the art of breeding more exact. By suitable crosses it ought to be possible to combine the most desirable characters in one variety. A good example is given by the work of Biffen himself on the production of rust-resistant wheats.

In England the cultivated wheats have a long growing period, heavy yield, and good standing qualities; but most of them usually suffer some loss, perhaps 10 per cent. of the total yield, from the rust fungus, *Puccinia glumarum*. To

remedy this defect, search was made for a variety resistant to the disease. Wheats collected from all over the world were grown at Cambridge and were found to differ greatly in their susceptibility to rust attack. Crosses were then made between a highly resistant and a susceptible form, and it was found that resistance was inherited as a Mendelian recessive. Accordingly, one of the best all-round English varieties, Squarehead's Master, was crossed with a resistant Russian variety which was itself useless for cultivation under English conditions since it gave very poor crops ; and from this cross a new variety, Little Joss, combining the good qualities of Squarehead's Master with the rust resistance of the other parent, was produced. The cross high yield, good standing, and susceptibility to rust, × low yield, poor standing, and resistance to rust had given a new form with the characters high yield, good standing, and resistance to rust combined—a typical example of recombination of characters following a cross.

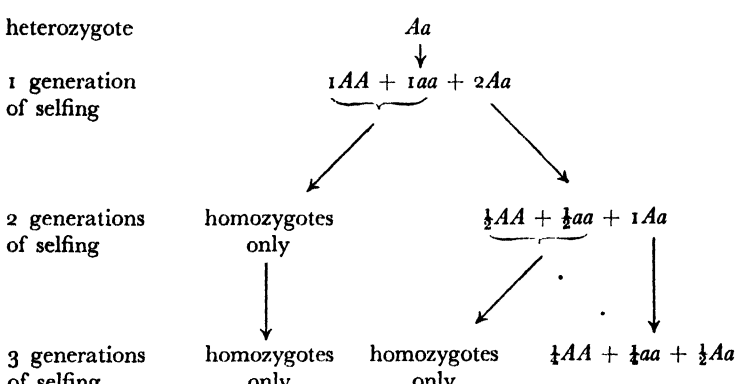
This method of improving plants is a valuable one ; though it has definite limitations since it gives no means of producing entirely new characters in a crop. It does mean that attempts can be made to obtain a variety in which every useful character—yield, standing power, quality, and so on —reaches its maximum development ; but if all varieties are, shall we say, susceptible to frost, then it is not likely that a frost-resistant form can be created.

To bring about improvement, it is necessary first to collect as many varieties as possible ; next to choose those in which the wanted characters are most highly developed ; and lastly to combine these characters if possible in a single variety, by the appropriate crosses. In practice it may be found that success is easiest in countries to which a crop has only recently been introduced, since in these circumstances it is not likely that the variety best adapted to local conditions already

exists. In other cases improvement is sometimes more difficult, unless scope is given by a change in the conditions of cultivation, or a demand for produce of a different quality from that already found.

Mendelian theory requires that individuals homozygous for any factor should always breed true for whatever character this factor may determine. Peas homozygous for *T*, for example, should breed perfectly true to tallness. Consequently, individuals that are homozygous for all their factors, if they can be obtained, should breed perfectly true for all characters.

This possibility is most easily realized in self-fertilized plants, such as peas, wheat, barley and so on. When a heterozygote, *Aa*, is selfed half its progeny will be homozygotes, *AA* and *aa*, and half heterozygotes again. On continued selfing the homozygotes breed true and the heterozygotes again give 50 per cent. of homozygotes, so that the proportion of heterozygotes in the population gets less and less.



The proportion of heterozygotes in each generation can easily be worked out if it be assumed that homozygotes and heterozygotes have the same number of offspring. Under

these circumstances, the number of heterozygotes is halved in each generation, so that after n generations of selfing the proportion of heterozygotes would be $(\frac{1}{2})^n$. Thus, after 10 generations only about one plant out of every thousand would be a heterozygote.

If the original individual were heterozygous for many factors the process is naturally rather slower. Algebraically, it can be shown quite easily that if m be the number of factors the proportion of plants homozygous for all factors after n generations would be :

$$\left(1 - \frac{1}{2^n}\right)^m$$

If there were 10 factors, then nearly three-quarters of the plants would be homozygous for all factors after only 5 generations of selfing.

In the case of characters such as tall and dwarf in peas, purity of type is not difficult to recognize ; but if the reasoning given above is correct it should be possible to obtain perfect purity, at any rate in self-fertilized plants, even in those less distinctive characters that can only be accurately defined by measurement. This principle forms the basis of Johannsen's experiments on the effect of selection for seed weight in the " Princess " bean, a variety of the self-fertilized *Phaseolus vulgaris*. He found that this commercial variety consisted of a mixture of closely similar types, which themselves bred absolutely true and were called by him *pure lines*. By selecting single plants from the commercial variety he was able to establish a series of these pure lines, each with a different average seed weight ; but once this had been done further selection had no effect, since within the pure line the different individuals were genetically identical, or, in Mendelian terms, they were all completely homozygous.

In the actual experiment, seeds of the commercial variety were sown singly and each plant obtained was harvested separately and its seeds counted and weighed. The seeds of these different plants were then sown in separate plots, one plot for the progeny of each plant. Each of these plots contained a pure line, and within one of these lines it made no difference whether the plant with the heaviest or with the lightest seed was selected: in either case the progeny had seed of the same average weight. In all, 19 lines were established, their average seed weight varying from 35·1 to 64·2 centigrammes. Selection was continued for 6 generations, from 1902 to 1907, in the line with the heaviest seed weight; the heaviest and lightest seeds within the line being sown separately each year.

The selection was without effect:

	Mean Weight of Selected Seeds.		Mean Weight of Progeny Seeds.	
Year.	Lightest Seeds.	Heaviest Seeds.	From Lightest Seeds.	From Heaviest Seeds.
1902 . .	60 cg.	70 cg.	63 cg.	65 cg.
1903 . .	55 ,,	80 ,,	75 ,,	71 ,,
1904 . .	50 ,,	87 ,,	55 ,,	57 ,,
1905 . .	43 ,,	73 ,,	64 ,,	64 ,,
1906 . .	46 ,,	84 ,,	74 ,,	73 ,,
1907 . .	56 ,,	81 ,,	69 ,,	68 ,,

As the table shows, the average weight of the progeny seeds varied from season to season, since some years were more favourable than others. In 1900 the value was only about 56 cg., in 1903 well over 70 cg. But this variation is fortuitous, and in no year was there any significant difference between the figures in the last two columns for the progeny of the lightest and the progeny of the heaviest seeds from the previous year's harvest. An experiment with the pure line having the lightest seeds gave a similar result.

It will be clear that Johannsen's discovery of pure lines supports the view that it should be possible to obtain in-

dividuals that are homozygous for all factors. Within a pure line plants will differ in average seed weight, because it is impossible to grow different plants under absolutely identical conditions—some will be better situated than others and will therefore be likely to bear heavier seeds; but whether the plants have heavy seeds or light seeds their progeny will have the same average seed-weight. In other words, chance differences due only to variations in the environment are not inherited. This conclusion, which agrees with the Mendelian principle, lies at the basis of modern genetics.

In cross-pollinated plants selection alone will not establish a pure line; self-fertilization must also be enforced if possible. In America, the self-fertilization of maize has been carried out for as many as 10 or 20 generations; and within the different lines uniformity increases, especially at first, in a very striking way; though it is not likely that true pure lines would be established within this period. There is no reason to doubt that the same principles apply as in self-pollinated plants, but exact experiments are not easily made because it usually happens that, when self-fertilization is enforced in plants that are normally cross-fertilized, there is a progressive loss of vigour as uniformity increases. The reason for this is not well understood, but it seems that in cross-fertilized species, such as maize, there is a definite vigour, " hybrid vigour " as it is usually called, associated with the heterozygous state.

Hybrid vigour has never been accurately defined, but is one of those biological conceptions which is probably quite valid despite the difficulty of defining it. It is an expression of the experience that when two strains obtained by several generations of self-fertilization are crossed together, the F_1, except perhaps in the case of plants that usually are self-fertilized, is often greatly superior to its parents in qualities

such as size and robustness of growth, the amount of seed it produces, resistance to disease, capacity to withstand adverse conditions, and so on. In maize, the F_1 between two inbred lines sometimes shows remarkable productive powers ; and in parts of the U.S.A. this has been exploited commercially, hybrid seed being distributed to the farmers. Though the cost of producing this seed is high, it is said to be repaid by the high yields the farmer is thereby able to obtain.

In bi-sexual animals it is manifestly impossible to obtain a pure line, since the two parents must differ at least in sex and whatever factors may be associated with sex. Occasionally, however, by a special mechanism, two, or sometimes more, genetically identical individuals may be obtained.

This occurs, for example, with the so-called identical twins which are not uncommon in man. These are derived from the same fertilized egg-cell, which instead of giving a single individual, separates into two identical halves, each of which develops into a complete embryo. From the way they arise these twins ought to be genetically identical, that is, to have exactly the same genetical factors, though they do not correspond exactly to a pure line, which consists of individuals that are homozygous as well as being genetically the same. It is, in fact, well known that identical twins are so alike in appearance that they cannot usually be distinguished from one another, and that their resemblance extends to their behaviour and mental characters. In the usual case the resemblance is undoubtedly aided by the fact that they have the same home and would seek the same occupations—since every motile animal seeks the environment that suits it best, and identical animals would want the same conditions. A few cases are known, however, in which identical twins have been reared apart, and these,

except for a few small differences that would be expected from their different environments, are still found to be alike.

In some animals, by continued inbreeding, brother and sister mating for example, it is possible to establish a strain which will be approximately pure for all the characters not associated with sex. Thus, in the guinea-pig, brother × sister mating has been continued for 30 generations, giving different lines that were all remarkably uniform. As with maize, a noticeable loss of vigour occurred, in some lines more markedly than others ; and the F_1 hybrids from crosses between different lines were very vigorous. Hybrid vigour in animals is sometimes utilized in ordinary agricultural practice. Thus, in producing beef or mutton the first crosses between two breeds are often used, in preference to the pure breeds themselves, on account of their capacity for quick growth ; and the soundness of the policy is attested by the success first crosses achieve at fat-stock shows. The classic example of hybrid vigour, however, is the mule, which has long been famed for its hardiness.

But although the fact of hybrid vigour sometimes makes inbreeding difficult to carry out, the experiments described evidently confirm the expectation, based on Mendelian theory, that it should be possible to establish lines, or strains, that breed absolutely true for all characters. Mendel's work showed that the units transmitted from one generation to the next, the factors, are constant ; and this constancy is confirmed by the existence of pure lines. In both cases the experiments have been carried out on a large scale, so that it seems unlikely that exceptions to this conclusion will be frequent.

The pure line theory is often tested in practice by the plant breeder since it is the principle upon which he bases his efforts to breed improved varieties of self-fertilized

plants. For unless there is some special virtue in a mixture, the aim of the breeder of these plants should be to produce the best pure lines. As Johannsen showed, the ordinary commercial variety is likely to be a mixture of different pure lines, closely similar in appearance but not equal in the less obvious characters such as yield or quality of produce. When this is so, improvement can be brought about by selection alone. Accordingly, the seeds of single plants, barley for example, are grown separately in small plots, and the different lines are judged or tested for yield and quality, for several generations if need be. The best of them is then selected and multiplied for distribution. The selection ought to be completely uniform—itself an advantage in crops such as barley where uniformity of grain is a valuable feature—and further selection would have no effect. Constant, accurate observation of these selections, whether on large or small plots, is part of the normal routine of such work and strongly supports the soundness of the pure line principle.

So far as is known, this principle was first used in breeding by Le Couteur, of Jersey, who published his experiments and reflections on the matter in 1836. In his day crops must often have been far more mixed than they are now, and a Spanish professor of botany, La Gasca, who was interested in the classification of wheat, pointed out to him that his wheat fields instead of being pure contained quite a mixture of types, differing from one another in hairiness, or in colour of grain, state of ripeness and so on. Le Couteur was profoundly impressed by this and concluded that he could not possibly be getting the best yield or quality of grain from such mixtures, that he ought to find the best type and grow only that. He first tried to do this by selecting ears that seemed alike and growing their progeny together; but after a few years concluded that he would

never gain his end in this way, and that he must select only single ears or grains as the starting-points for his new varieties. This method he adopted, and obtained much better yields in consequence.

Le Couteur's achievements may be contrasted with those of Hallett, who, in the middle of the last century, tried to improve the varieties of cereal crops, such as wheat or barley, by carrying out selection on a totally different principle. Like Le Couteur he selected single plants. But he did not attempt to choose the best strain by testing one against the other. Instead, he tried to get a continuous improvement year after year by growing all his plants with ample space under the best possible conditions, and choosing the highest yielding plant each year, hoping that the luxuriant growth induced by these methods of cultivation would tend to be passed on to the offspring. With our modern knowledge we should not expect these methods to have effect; and this seems to have been the case. His new wheats did not give high yields, and for this reason never entered into general cultivation. On the other hand, his barleys were widely grown on account of their outstanding uniformity, for which the maltster is willing to pay a high price that will more than offset the disadvantage of a small yield. This result is exactly what might have been expected to follow his selection by single plants.

The results of later breeders are equally significant. A new cereal variety is normally descended from a single plant, and in the early stages of multiplication large numbers of the offspring of this plant are critically examined; in this way, indeed, irregularities in inheritance have sometimes owed their discovery to the breeders' observation. The general experience of breeders appears to confirm the pure-line principle, and this is strong evidence for its truth.

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CHAPTER III

MENDELISM (*continued*)

ALL the cases of inheritance worked out by Mendel were alike in that the transmission of each character depended upon only one factor. When his work was rediscovered, however, various complications were soon found; and among them, as Bateson showed, that the development of a character, instead of depending upon only one, might depend upon two or more factors, transmitted independently of one another. This adds greatly to the possible complexity of inheritance; but the principle is still simple, and once it has been grasped the different cases may be worked out quite easily as they arise. Details, such as the actual ratio obtained in any one case, have no special importance.

An example of bifactorial inheritance is a case in the sweet pea in which two white races crossed together give a red flowered F_1. The two races look identical but they are evidently different genetically, since the product of crossing them is different from that obtained when either is selfed. Further analysis showed that red flowers are produced when two independent factors, *C* and *R*, are present in a plant together; and that when only one or none is present the plant has white flowers. The two parental races had the formula *CCrr* and *ccRR* respectively, and both therefore were white. The F_1 is *CcRr*, and is red flowered because *C* and *R* are present. F_2 was found to consist of red-flowered and white-flowered plants in the ratio 9 : 7. How this comes about will be worked out in detail.

parents $CC\ rr \times cc\ RR$
(white) (white)

F_1 $Cc\ Rr$
(red)

F_1 gametes $1CR + 1Cr + 1cR + 1cr$ or $(C + c)(R + r)$

F_2 $(C + c)^2(R + r)^2$
or $1C^2R^2 + 2C^2Rr + 2CcR^2 + 4CcRr + 1C^2r^2 + 1c^2R^2$
(red) (red) (red) (red) (white) (white)
$+ 2Ccr^2 + 2c^2Rr + 1c^2r^2$
(white) (white) (white)

total 9 red : 7 white

The F_2 can also be worked out by the chessboard method. The four female gametes *CR*, *Cr*, *cR*, and *cr* are written out four times in horizontal rows. To each member of the first row the male gamete *CR* is added, to the second *Cr*, to the third *cR*, and to the fourth *cr*. This gives, in separate squares, the zygotes obtained from the 16 possible matings of the four different kinds of gametes.

1 *CR* *CR*	2 *Cr* *CR*	3 *cR* *CR*	4 *cr* *CR*
5 *CR* *Cr*	6 *Cr* *Cr*	7 *cR* *Cr*	8 *cr* *Cr*
9 *CR* *cR*	10 *Cr* *cR*	11 *cR* *cR*	12 *cr* *cR*
13 *CR* *cr*	14 *Cr* *cr*	15 *cR* *cr*	16 *cr* *cr*

If the squares are counted up it will be found tnat nine of them, shaded, contain both *C* and *R*, while the remaining seven lack one or both of these factors. F_2 will therefore contain red- and white-flowered plants in the ratio 9 : 7.

When the F_2 is self-fertilized it is found that the whites all breed true ; and that some of the reds breed true, but most of them split up again into red and white. This can easily be worked out from the composition of the F_2 plants, as shown by the chessboard. The results may be summarized as follows :

F_2	Number of Square	F_3
1 *CCRR*	1	red only
2 *CCRr*	2, 5	3 red (1*CCRR* + 2*CCRr*) : 1 white (*CCrr*)
2 *CcRR*	3, 9	3 red : 1 white
4 *CcRr*	4, 7, 10, 13	9 red : 7 white, like the F_1
2 *Ccrr*	8, 14	All these breed true to white
2 *ccRr*	12, 15	
1 *CCrr*	6	
1 *ccRR*	11	
1 *ccrr*	16	

It will be seen that the whites, though all breed true, are of several different kinds genetically, and will give various results when crossed among themselves. *cc rr*, for example, will give nothing but white if crossed with any other white ; *Cc rr* × *ccRR* will give an F_1 consisting of 1*CcRr* : 1*ccRr*, that is to say, red-flowered and white-flowered plants in equal proportions ; and so on.

In cases like this, in which two independent factors are needed for the development of a single character, the two factors are said to be *complementary*. It has been found that there are various ways in which such factors may interact, with the result that bifactorial inheritance follows a number of different schemes each giving a different ratio. Some of these will be briefly described.

An interesting example is the inheritance of the types of comb found in poultry. Rosecomb and peacomb are both dominant to single. Rose differs from single by one factor, pea differs from it by another factor. This may be repre-

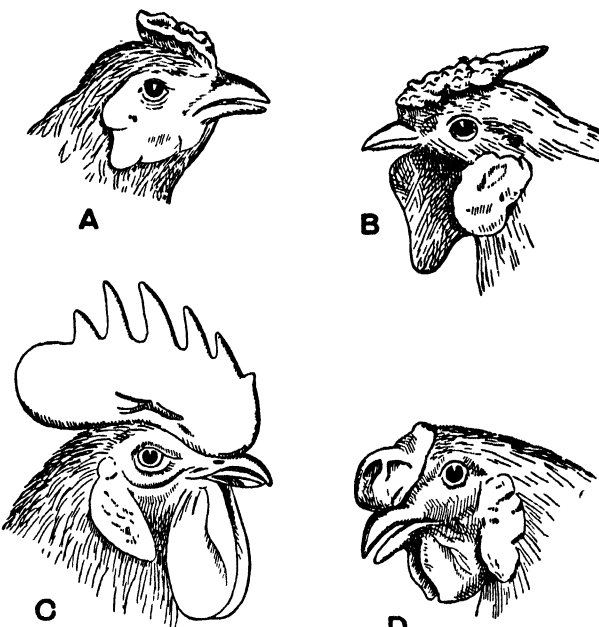


FIG. 1.—Comb types in poultry. A, pea; B, rose; C, single; D, walnut.

sented as follows: single = *aa bb*, rose = *AA bb*, pea = *aa BB*. When rose and pea are crossed together a fourth type of comb, walnut, is produced, owing to the interaction between *A* and *B* when both are present together. The F_1 birds, *Aa Bb*, are themselves walnut; and in F_2 there will be a ratio of 9 walnut, containing both *A* and *B* : 3 rose, containing *A* but not *B* : 3 pea, containing *B* but not *A* : 1 single, containing neither *A* nor *B*. This ratio

can easily be verified by referring to the chessboard on page 29 or the formulae on the same page and substituting *A* and *B* for *C* and *R*. The ratio 9 : 3 : 3 : 1 has already been met with in describing the inheritance of two independent characters, each due to a single factor. The case of the poultry combs gives the same ratio because here also there are two independent factors which behave in exactly the same way except that both have an effect upon one character instead of upon two different characters.

Another fairly common bifactorial ratio is 12 : 3 : 1. It can occur when one character is obscured by another. A good example is provided by the inheritance of colour of husk in oats. This may be either black, grey, or white ; and in appropriate crosses both black and grey give unifactorial inheritance when crossed with white, the F_2 ratios being 3 black : 1 white, or 3 grey : 1 white. Representing black by *B* and grey by *G* it is clear that the plants *BB gg* and *bb GG* will be black and grey respectively. Crossed together they will give an F_1 with the formula *Bb Gg*, heterozygous for both black and grey. These two characters are inherited independently, so that the F_2 will consist of 9 having both black and grey pigment : 3 black but not grey : 3 grey but not black : 1 white. The husk of oats containing both black and grey pigment cannot be distinguished from that containing black alone, so that the visible ratio will be 12 black : 3 grey : 1 white. Black, in such a case, is said to be *epistatic* to grey. The phenomenon was first found by the French zoologist Cuénot, in mice ; but the example was not quite so simple as the one described here for oats.

It will sometimes be found, both in plants and in animals, that there is a dominant factor, *I*, which inhibits the development of colour. Thus, in fowls there is one that only allows colour production in the iris of the eye, and occa-

sionally elsewhere, the plumage being white. A bird of the formula *BB II* is therefore a dominant white, in which the black colour, due to *B*, is prevented from appearing by the factor *I*. There is also a recessive white or albino, *bb ii*, which is white because it lacks the factor *B*, or any other colour factor. This form has no pigment either in the eyes, which appear pink, or in the plumage; and is thereby distinguishable from the dominant white. When these two whites, *BB II* and *bb ii*, are crossed, black birds, such as *BB ii*, appear in F_2 in the ratio 13 white : 3 black, the 3 blacks being those that contain *B* and are without *I*. The details can be worked out quite easily with the help of the chessboard.

Other bifactorial ratios are possible, but enough has been said to indicate the kind of complications that may be found when two factors are concerned in the production of one character. Still more complicated ratios arise when three or more factors are involved. Examples have been worked out chiefly in coat colour in animals and flower colour in plants, since in many species a number of colours exist which are sufficiently definite for quite complicated crosses to be classified without great difficulty. In the sweet pea, besides the two factors for red flower colour, *C* and *R*, there is a third factor which changes the flowers from red to purple, and other factors may modify them still further.

An F_1 heterozygous for 3 factors will give an F_2 having the composition :

$$(A + a)^2(B + b)^2(C + c)^2$$

With n factors the F_2 will be given by :

$$(A + a)^2(B + b)^2(C + c)^2 \ldots \text{ to } n \text{ factors.}$$

In many of the examples that have been described the presence of a character has been dominant to its absence :

coloured, the presence of pigment, dominant to white, the absence of pigment. This is found quite commonly, and led to the supposition, first put forward by Bateson and Punnett in 1905, that a dominant factor, *C* for example, actually represents a substance which is present in the dominant type but absent from the recessive, *cc*. It would probably be a mistake, at any rate in most circumstances, to regard the recessive factor *c* as representing complete absence; and with our present ignorance of the nature or method of working of Mendelian factors suggestions of this kind must be looked upon as speculative; but it is quite likely that in some instances the suggestion contains a large element of truth.

Support for the idea is given by the existence of *multiple allelomorphs*, series of three or more factors which are all allelomorphic to one another. A good example occurs in the Chinese Primrose, *Primula sinensis*, where there are three multiple allelomorphs controlling the size of the coloured eye of the flower. In the variety Queen Alexandra with the factor Q_1 there is no eye, the lobes of the petals having the same colour over their whole area. In normal-eyed varieties, having the factor Q, the base of the petal lobes is yellowish in colour, producing a more or less circular area, with a distinct colour, in the centre of the flower at the top of the corolla tube. In a third type, Primrose Queen, carrying the factor q, the eye is much larger than normal. There are therefore three eye-types: large (q), normal (Q) and none (Q_1). Whichever types are crossed simple Mendelian segregation occurs: large × normal gives only large and normal; large × none gives large, intermediate and none; and normal × none gives normal, intermediate and none. In other words, the three types are represented by three factors which are all allelomorphic to one another.

		F1	F2		
large qq × normal QQ	→	Q	→ qqq (large)	Qq (normal)	QQ (normal)
large qq × none Q_1Q_1	→	Q_1q	→ qq (large)	Q_1q (intermediate)	Q_1Q_1 (none)
normal QQ × none Q_1Q_1	→	QQ_1	→ QQ (normal)	QQ_1 (intermediate)	Q_1Q_1 (none)

The effects of the three factors—q, Q and Q_1—differ from one another in a quantitative manner, the amount of eye they produce, and this is what would be expected if the factors themselves differed quantitatively from each other, the recessive q lacking something that is present in Q and present to a still greater degree in Q_1.

Similar cases are found in other organisms. In mammals there are series of multiple allelomorphs determining coat colour, in which the effects produced form a graded series from light to dark. Cases of this sort are most easily visualized by supposing that the factors concerned may be arranged in a graded series in which the different members differ from one another in a quantitative way. Sometimes, however, as we shall see in a later chapter, this interpretation is not possible.

The inheritance of colour differences, which usually give favourable material for genetical work, has now been studied in a number of animals and plants. In mammals, many of the different species have similar series of coat colours, and it appears that the genetical relationship between the various types is the same in these different species. Thus, a black variant, recessive to the normal and differing from it by one factor, has been found in the mouse, Norway rat, black rat, guinea pig, rabbit, and cat; and a recessive yellow in the black rat, deer mouse, guinea pig, rabbit, dog, cat, and ferret. In both guinea pig and rabbit, there is a series of multiple allelomorphs that have

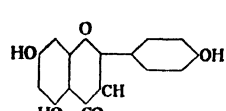
the effect of diluting the normal pigment; and some members of the series appear to be present in the mouse, Norway rat, and perhaps some other animals. Other instances might be given.

These observations undoubtedly suggest that the various species in question have a number of factors for coat colour in common. They do not definitely prove it, because cases are well known in which, in one and the same species, different factors produce the same effect: in the sweet pea, for example, a change from *C* to *c* or from *R* to *r* will give white. It cannot, therefore, be argued that in different species similar changes need be due to the same factor. Nevertheless this conclusion is probably true in the case in question, at any rate for many of the changes, since the similarity extends over many variations and includes the special relationship of multiple allelomorphism. The general tenor of the observations certainly makes it very probable that many of the factors for coat colour in this series of related species are identical.

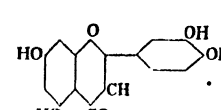
The genetics of flower colour has also been studied extensively, and it has been found that the *C R* type of inheritance, that is to say, the existence of two separate factors for the production of colour, is common. But in some cases, of which *Antirrhinum* is one, the situation is a little different from that in the sweet pea because instead of both the types *CC rr* and *cc RR*, which lack one of the necessary factors for colour, being pure white, only one is white, and the other is either ivory or yellow.

The widespread nature of the *C R* type of inheritance for flower colour suggested the possibility that some comparatively simple biochemical mechanism might be responsible for colour production in all these cases, so that attention was directed to studying the chemistry of the pigments concerned.

Flower colour may be due either to the presence of coloured plastids, minute rounded bodies present in the cells of the petals, or to soluble pigments dissolved in the cell-sap. The plastids may contain either xanthophyll, a yellow pigment, or carotin, red; but consideration will only be given here to the effects produced by the soluble pigments. Chemically, the soluble pigments have been found to belong to one or other of two series, anthocyanins and flavones; and the cell-sap may contain either an anthocyanin or a flavone, or both may be present together. The way in which these pigments produce different colours has been worked out chiefly in the genera *Dahlia* and *Antirrhinum*, in which the colour-range—white, various shades of yellow or red, magenta, and so on—is very wide. When these types are studied it is found that they may be grouped into two series. The ivory series contains the flavone known as apigenin, and consists of colours ranging from ivory to magenta and deep purple. The yellow series is produced when the flavone luteolin is present instead of apigenin, the colours obtained varying from yellow to orange and deep scarlet. The two series differ from each other genetically by a single factor, and the two flavones by which they are distinguished are simply related to one another chemically, as indicated by their structural formulae.



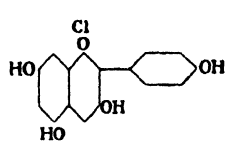
Apigenin



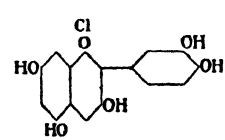
Luteolin

How the Mendelian factor concerned determines which of these two substances is formed is not known, but it is interesting to find that a simple genetical relation between the two series is reflected so closely by an equally simple chemical difference.

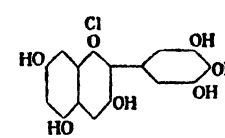
By themselves, the flavones only give colours ranging from ivory to deep yellow. The additional factor in producing the final colour in *Dahlia* is the anthocyanin which may be present in the cell-sap as well as one of the flavones. With the flavone apigenin the anthocyanins give the magenta series of colours, according to which anthocyanin is present and other circumstances, and with the flavone luteolin they give the orange series. The exact colour produced in any one case, however, depends upon a variety of circumstance and it is not possible to make simple generalizations like that concerning the differences associated with the two flavones. Nevertheless, certain definite statements can be made. Thus, in some genera the colour produced is different according to which anthocyanin is present—the blue cornflower, for example, contains cyanidin and its pink variety contains pelargonidin—and a single Mendelian factor will determine which anthocyanin is produced. Moreover these different anthocyanins are simply related chemically.



pelargonidin chloride



cyanidin chloride



delphinidin chloride

In general, however, matters are more complicated. On the one hand, the same colour may be produced in different plants by different anthocyanins. On the other, it may happen that in the same plant the same anthocyanin will give different colours according to the circumstances. Thus, in *Primula sinensis* there is a factor *R* which makes the cell-sap more acid and so may change the colour of the flowers from blue to magenta; for the anthocyanins are indicators of acidity.

So far nothing has been learned about the more fundamental problems concerned in the relation of genetics to

biochemistry. It is interesting to know that a single factor determines whether apigenin or luteolin is produced, but since these substances are quite likely to be mere by-products of cellular activity the knowledge tells us nothing about the chemical nature or activities of the genetical factor itself. Nevertheless, the researches described have yielded interesting results and may provide a line of approach to the more fundamental problems still to be solved.

Cases of multifactorial inheritance, of which those described for colour in animals or plants are only a particular example, make it easy to understand a phenomenon that was noticed by the early hybridizers, namely that a cross between two domestic varieties will sometimes give the wild type. This reversion, as it has often been called, occurs because many cultivated varieties of plant or animal are recessive to the wild; and when two of them are crossed it may happen that the factors necessary to produce the wild-type characters are brought together. In the sweet pea, for example, in which the factors *C* and *R* must both be present for colour to be formed at all, and *B* changes the colour from red to purple, a cross between two white-flowered races such as *CCrrBB* and *ccRRBB* gives an F_1 *CcRrBB* with purple flowers like the wild pea.

In one or other of the many species, plant or animal, in which Mendelian inheritance has been studied, factors affecting development in almost every conceivable way have now been found. In plants, mention may be made of characters such as resistance and susceptibility to disease attack, first discovered by Biffen at Cambridge for the relation of wheat to the attacks of rust fungus; shape of leaf, as in *P. sinensis* and other plants; the difference between the long and short-styled forms of *Primula*, that is to say the two forms that ensure cross pollination in these species; the fertility of the lateral florets in barley; shape of pollen

grains in the sweet pea. Equally diverse examples may be found in animals, and it may happen that a factor has such a serious effect on development that it causes the death of individuals homozygous for it. A factor of this kind is said to be *lethal.*

Lethal factors were first brought to light through the work of Cuénot on the inheritance of yellow coat-colour in mice. Yellow is dominant to the ordinary wild grey or agouti and only one factor is involved. This is shown by the fact that heterozygous yellows, Yy, when mated with agouti, yy, gave 177 yellows, Yy, and 178 greys, yy, the 1 : 1 ratio expected from such a mating. The heterozygotes, Yy, when mated together should have given some homozygous yellows, YY, but when the experiment was performed it was found that all the yellow mice obtained were heterozygotes.

Cuénot himself did not interpret this correctly. He argued that Y and y gametes were produced in equal numbers by the heterozygotes, and that y gametes could unite either with Y or with y; but that YY individuals were not formed because an egg-cell and a sperm both carrying Y were unable to unite. This reasoning is quite sound, and though no such case has yet been discovered there seems no reason why it should not. In the present example, however, further investigation showed conclusively that Cuénot's interpretation, which would indeed give a different ratio from the one he found, was wrong; that the heterozygotes, Yy, do produce egg-cells and sperms in the expected ratio 1 Y : 1 y, and that random mating occurs just as in any other

Yy (yellow)

↓

♀ gametes 1 Y 1 y

♂ gametes 1 Y 1 y

zygotes, by random mating, 1 YY : 2 Yy : 1 yy

(dies) (yellow) (grey)

case, giving 1 YY : 2 Yy : 1 yy. The reason for the unusual ratio is that YY individuals do not live. They die in the embryonic stage and may be found, disintegrating, in the uterus. The zygotes that survive until birth appear, therefore, in the ratio of 2 yellow : 1 agouti. If Cuénot had been right it is not difficult to see that the ratio found would have been 3 : 1.

Lethal factors have now been found in a wide range of organisms, both plant and animal. They are of some practical importance in the breeding of domestic animals, since they seem to be fairly common in some strains, at any rate in cattle, where they show their effect in cases of abortion, still-born monstrosities, and so on.

One of the earliest, and best-known, examples is that of the Dexter, a breed of Irish cattle, like the Kerry in appearance but smaller. Breeders of Dexters encountered difficulties from two causes. The first was the birth of a considerable proportion of still-born, abnormal, calves ; and the second the constant production of individuals resembling the Kerry. It is now known that the Dexter actually owes its distinctive differences from the Kerry to the possession of a lethal factor, *l*, for which it is heterozygous. The relation is Kerry = *LL*, Dexter = *Ll*, still-born " bull-dog " calf = *ll*. The Dexter therefore produces 25 per cent. of Kerries, and 25 per cent. of bull-dog calves.

Several other lethal factors have also been found in cattle ; and in some cases it seems that a famous sire has been heterozygous for such a factor, which has consequently become widespread within a breed. Lethal factors have also been found in other domesticated animals.

The diversity of the characters, both in animals and in plants, for which Mendelian inheritance was proved to occur, soon encouraged the earlier genetical workers to contend that all inheritance would prove to be Mendelian.

It is now known that there are cases, one of which is the transmission of chloroplasts, the bodies that contain the green colouring matter of plants, that fall outside this scheme ; but those discovered so far are very few, and do not seem to have much general importance. If these be neglected, the case for the Mendelian claim is certainly strong. In one organism or another Mendelian analysis has been successfully applied to characters of the most diverse nature—whether physiological as in the case of resistance to disease in wheat, or morphological as with the long hair of the Angora rabbit—and to variations affecting any part of the individual.

But in one respect the cases so far given do not represent all the possible kinds of variation. For, an essential part of Mendelian analysis is that, as in the case of the dwarf and tall sweet peas, there should be no doubt about the class to which each individual belongs. Mendel was easily able to classify his peas as either tall, about 5 or 6 feet high, or dwarf, from about 9 inches to 2 feet in height. There were two easily-defined types. If height had varied continuously from, say, 4 feet to 6 feet, classification would have had to be quite arbitrary and of uncertain meaning.

In other words, all the cases for which Mendelian inheritance has so far been described are examples of *discontinuous variation*, cases in which the different types are sharply separated from one another and could not easily be confused. In none was variation *continuous*, as with height in normal European man, where there is no clear line between one type and another. Before we can decide whether Mendelian inheritance is universal, certain definite questions must be answered. In the first place, whether it is right to conclude that variation is of two kinds, continuous and discontinuous : the first of these applying to all measurable, or quantitative, characters such as seed-weight in beans and body-weight

in man, in which there is every gradation from one extreme to the other ; and the second to qualitative characters such as colour, and other cases in which varieties fall into sharply separated classes. In the second place, if this distinction is a valid one, whether Mendelian analysis can be applied to examples of continuous variation.

It may perhaps be granted that, in view of the many cases in which it has been successfully applied, all cases of discontinuous variation would prove to have Mendelian inheritance if they were analysed. But continuous variation might be in a separate category. It might easily be argued that any kind of variation that is continuous would not be likely to follow a system of inheritance based on the existence of constant unit differences.

An answer is provided by Johannsen's work on pure lines, which is the principal evidence against the justice of this view. The whole matter, however, is one of fundamental importance and must be considered more critically and in detail.

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CHAPTER IV

VARIATION

IT rarely happens that all the individuals making up a plant or animal population are alike. Usually, variation occurs. The amount of variation—the degree of difference between individual members—depends upon a number of factors, such as the species examined, and the locality from which it was collected. The domestic dog is more variable, in shape, size, colour, and so on, than the cat; wheat more variable than rye. There are more varieties of potato in Chile and Peru than there are in Britain. With the help of the Mendelian principle, an analysis of variation in a population can be made with an accuracy that was not hitherto possible.

In the first place, it will be found that a frequent reason for diversity is that a small number of variations have been combined in all possible ways. In the rabbit, the four colours, agouti, black, yellow, and tortoise-shell, are changed by loss of the black factor to cinnamon, chocolate, cinnamon yellow, and orange. The addition of the blueing factor to these eight types gives eight more colours, such as blue-agouti, and lilac; a total of sixteen colours in all. With five pairs of factors, each pair being responsible for two colours, recombination will give 2^5, or 32, different types; and ten pairs of characters would give 2^{10}, or over 1,000 types. Once this is realized it becomes possible to express the variation in a diverse population much more simply.

It is important to understand that this method of classifying organisms, which is essential in genetical work, rests on the principle of considering each character separately instead of taking all characters at once and trying to fix the degree of general resemblance. The individual is not considered as a whole, but as being made up of a number of separate and independent units. This way of approach is the natural outcome of Mendel's conclusions ; indeed, without it the problem of inheritance would have remained unsolved. The first task in any genetical analysis, therefore, but one which may sometimes be difficult, is to define the separate characters in which variation occurs. Examples of pairs of alternative characters, such as tall and dwarf, have been given already ; an obviously more difficult case is a character like intelligence in man, which is difficult to define and is probably made up of several independent attributes, so that a simple classification into intelligent and unintelligent would have little value.

Next, it is necessary to distinguish, as far as possible, between variations that are genetically caused and those due to the environment, or *fluctuations* as they are usually called.

An example of fluctuation is the variation within a pure line of beans, in which differences in seed-weight are due to the slight dissimilarities in the conditions under which the plants grew. Fluctuation may indeed be defined as the variation occurring within a pure line, or other genetically identical material, as a result of inequalities in the environment. It is not inherited, as Johannsen first showed. In practice, it is often most easily recognized by observing variation within a single individual, as, for example, the variation in intensity of colour among different flowers of the same plant, since the different parts of the same individual are with rare exceptions genetically identical. This fact will be more readily comprehended after cell division

has been described, but even at this stage it might be expected, since Mendelian inheritance suggests that the whole individual, being descended from a single fertilized egg-cell, would have the same factorial composition in different parts.

Examples of fluctuation are constantly met with in the course of genetical work, and it is one of the first tasks of the genetical worker to distinguish variation of this kind. A good illustration is given by the development of awns in wheat. Most varieties of wheat belong to one of two genetically different classes: fully awned (fig. 2*a*); and tipped, or awnless as they are usually called (fig. 2*b*, *c*). There is some variation within each of these classes: in the first the length of awn varies in different races, and in the second the length of the very short awns or tips, though usually only a few millimetres, may reach one or more centimetres, especially at the top of the ear (fig. 2*c*). But no confusion is likely to arise between the two, and the difference is inherited as a Mendelian unit, with awnless dominant to awned. The character is also subject to fluctuation, however; the degree of development of the awn being influenced by external factors such as time of sowing, whether spring or autumn. An extreme example is given by some varieties of macaroni wheat, *Triticum durum*, from Abyssinia. Sown in England in autumn or early spring, they develop awns of medium length (fig. 2*d*); but in a wet summer they produce from the base of the plant late shoots which come into ear a month or more after the normal shoots, and have no awn at all except for a short and somewhat thickened outgrowth (fig. 2*e*). A difference of this kind, if it were found on separate plants, might easily be mistaken for a genetical one; and is in fact quite as great as the normal difference between awned and awnless, caused by a Mendelian factor. Actually it is a mere fluctuation, and is not transmitted to

the next generation, as may be readily verified by sowing the seeds from awned and awnless ears in separate plots.

In the same way the young of most mammals suffer grave aberrations in development if they are not given enough

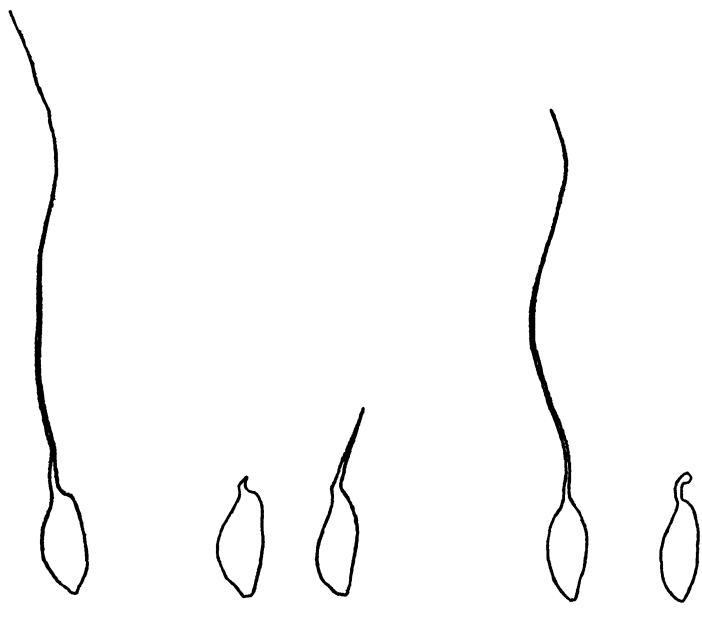

FIG. 2.—Variation in the development of the awn on the chaff of some different races of wheat.

a, from an awned variety ; *b*, from the bottom, and *c*, from the top, of the ear of an awnless variety , *d*, from a normal shoot of an Abyssinian variety, and *e*, from a late shoot of the same plant.

vitamin *D* ; but they do not transmit these defects to their offspring : the maldevelopment is a fluctuation, brought about by the environment entirely, and could have been remedied by a change in diet. There are, of course, possible complications. Thus, if the mother is starved of

vitamin, or some other essential constituent, she might be unable to supply enough for the normal development of the embryos; but this is not true inheritance. It should also be realized that the amount of vitamin needed is greater in some races than others, the difference being genetically caused.

Usually, as might be expected, fluctuation produces continuous variation, variation from one extreme to the other, like the variation in the seed-weight of beans belonging to a pure line; and when material is analysed genetically the experimenter is usually safe in assuming that well-marked discontinuous differences have a genetical basis. Occasionally, however, this is not the case.

Thus, in the wheat example, the normal and the late shoots are often produced with an interval in between; so that a well-marked discontinuity, simulating that due to a Mendelian factor, is found between different ears of the same plant. A somewhat similar case is presented by a race of *Primula*, which has white flowers if development occurs above 30° C. and red flowers with temperatures from 10° C. to 20° C. Other races have red flowers at any temperature, the difference between the two kinds being caused by a single factor. In animals also, as with the white winter coat of the mountain hare, the environment may determine the development of two apparently distinct forms. Again, the locust, *Locusta migratoria*, occurs in two phases, the solitary and the migratory, which are so distinct from one another that they were at one time believed to be distinct species. The change in form from one to the other is brought about by crowding.

These examples of fluctuations resulting in discontinuity are extreme cases, however; and it will usually be the case, as stated above, that discontinuous differences are genetically caused.

The principles underlying the genetical analysis of variation may now be summarized in the light of what has been said. In the first place, the individual is not considered as a whole ; each character is studied separately. It is then found that variation is either discontinuous, that is to say, the individuals fall into two or more distinct groups, or continuous, when no such separation appears to be possible. If the first be the case the difference probably has a genetical basis. It may be determined by a discontinuity in the environment—one group might have been exposed to infection by disease and the other not exposed—but with reasonable care these cases should usually be recognized without much difficulty.

If, on the other hand, variation is continuous, part of it will be fluctuation caused by small differences in the environment, but part may have a genetical basis : variation in the Princess bean was shown to be partly the result of fluctuation, and partly to be caused by the presence of genetically different pure lines.

In these cases of continuous variation, analysis is most easily carried out in self-fertilized plants. In these it is possible to isolate pure lines which breed true to different mean values ; so that, when one line is compared with another, variation is revealed as discontinuous. In a mixed population, however, as in the commercial bean variety, this discontinuity would be masked by fluctuation, with the result that apparent continuity is found.

In cross-fertilized plants, and in animals with separate sexes, the same principles apply but the situation is more complicated. However, even when fluctuation makes the variation seem continuous, the population will usually contain a definite number of genetically distinct types which in theory would breed differently, just as the various pure lines have different mean values. In self-fertilized plants every

individual is homozygous, as a result of the long-continued self-fertilization, and the different types can be isolated at once by growing the progeny of each individual separately. In animals and cross-fertilized plants the different genetical types cannot be directly isolated, since it is not likely that any individual would be homozygous for all factors ; and even if it were, the chance of its mating with an individual genetically identical with itself would be negligible. But there can be no doubt that the principles involved are exactly the same as those successfully employed in analysing self-fertilized plants.

Before dealing with continuous variation in more detail an alternative classification will be considered.

In the first place, a distinction may be drawn between qualitative variation, differences in the nature of a substance, such as that between two chemically different pigments, and quantitative variation, differences in the amount of a substance that is produced. The black and the chocolate mouse contain different kinds of pigment. They may also vary in the amount of pigment, that is, quantitatively. Size, body-weight, or the amount of protein in seeds, are all good examples of quantitative characters.

Another possibility is variation in shape ; that is to say, in the relation of one part to another, or in the relation between one dimension and another, for example the length and breadth of a leaf.

Finally, there may be variation in the number of repeated parts ; such as the number of digits or teeth in mammals, or the number of petals in a flower ; called by Bateson *meristic* variation.

There seems no doubt that the principles of Mendelian inheritance apply to all these categories ; though most of the examples so far given have been examples of qualitative variation. However, differences in shape have in certain

cases—the leaves of *Primula* for example—been shown to follow Mendelian inheritance; and the many instances where this demonstration would be difficult probably do not differ in principle from cases of continuous variation considered in more detail below.

Meristic differences usually characterize organisms too distantly related to be crossed, so that little is known about their inheritance. What is known does not suggest that they are genetically remarkable.

Quantitative characters clearly show simple Mendelian inheritance in some cases, as in the tall and dwarf sweet pea, but far less frequently than qualitative characters do. This is because it so happens that, while qualitative variation is usually discontinuous, falling into well-defined classes like coloured and white flowers, in quantitative characters it is very often continuous, with every gradation from one extreme to the other.

As suggested in the last chapter, continuous variation is not easy to analyse on Mendelian principles. When, as with tall and dwarf, there is no doubt about the limits of the classes into which the different individuals are put, genetical analysis is likely to be easy, whether an F_2 or a naturally occurring population is concerned. Otherwise it may be difficult; and it will in fact be necessary to discuss the genetics of continuous variation at some length.

Examples of continuous variation are chiefly drawn from quantitative characters, though the association is not invariable. An association of some kind is only to be expected; for it is a characteristic of the material world also that a substance may be either of one kind or another, giving discontinuity, but that the amount of it may have any numerical value, and therefore varies continuously. In living things, however, quantitative variation is often discontinuous. Thus, although height in man is one of

the commonest examples of a quantitative character that varies continuously, in the sweet pea tall and dwarf is the classical case of discontinuity with simple inheritance. Indeed, when the whole of the variation in man is considered there is a clear discontinuity between normal man and the dwarf some two or three feet high. The continuous variation found in normal man is comparable to the variation that occurs between different races of tall sweet pea. It is much influenced by the environment, and though not great in total amount can be measured with precision. No doubt there would be a similar variation in the amount of pigment present in a flower, but this would not be easily measured. While, therefore, quantitative characters may vary discontinuously, part of the variation found is likely to be continuous. The pure-line theory, and the analysis carried out earlier in this chapter, suggests that this continuity may be more apparent than real : that there may be hereditary differences present which are discontinuous, but are obscured by fluctuation ; for the degree to which any character develops is likely to be considerably influenced by the environment.

It will probably be admitted that qualitative differences, like colour and many other examples described in earlier chapters, and large quantitative differences, like those between tall and dwarf pea, are due to Mendelian factors. What we have still to discuss is whether the small quantitative hereditary changes, like height in European man, comparable in size with those caused by the environment, are also due to Mendelian factors.

The strongest experimental evidence that continuous variation may be explained in Mendelian terms is Johannsen's work on pure lines.

On Mendelian principles, completely homozygous individuals, breeding absolutely true, would be expected in self-

fertilized plants; and in these cases, if a character like seed-weight were affected by several factors with independent effects, we ought to find, as we do, different pure lines each breeding true to a different average seed-weight. The assumption that seed-weight is influenced by several Mendelian factors would seem to be reasonable, since little reflection is needed to show that characters of this kind are likely to depend upon a variety of independent circumstances, and therefore may well depend on many Mendelian factors. Two plants of equal capacity might bear many small seeds or fewer large ones. One plant might have better developed roots than another, and therefore be able to withstand drought more readily. The time of flowering might be different in different lines, so that the seeds of one line chanced to grow in better weather conditions than those of another. This last possibility shows very well how complicated the matter may easily be since the effect would differ in different seasons; it is indeed a common experience, in testing the yield of varieties of crop plants, that one variety is best in one season and another in the next.

For reasons such as these it will readily be granted that when a character can be measured with precision the degree of its development will almost certainly be found to depend upon a number of independent Mendelian factors, all contributing to the final result. Similarly it may be argued, to the same effect, that when development, as in the case of size, is known to be influenced by different environmental factors, it is only to be expected that it would be influenced by many genetical factors.

It is not easy to prove, however, in a specific case, that inheritance has been due to the cumulative action of many independent factors; for analysis is naturally difficult, if not impossible, whenever the effect of a Mendelian factor is less than that of the environment, and this will usually

be the case with variations of the kind we are now dealing with. Evidence for segregation of some kind or other is more easily provided.

Thus, when two varieties are crossed it is commonly found, if a large enough F_2 is grown, that the parental values for height, weight, or other characters, are recovered. In other cases the variation in F_2 goes beyond the values reached

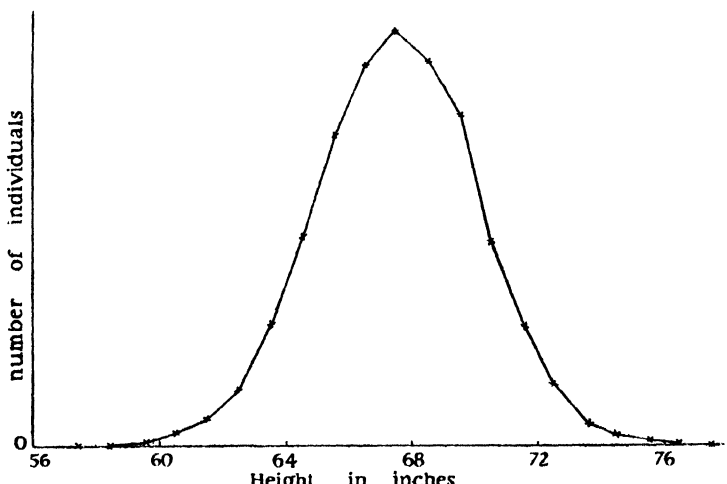

FIG. 3 (after Yule).

by the parent forms, and this would frequently be expected. Suppose, for example, that dominant factors increase stature and recessives diminish it ; then a cross of the type *AA BB CC DD ee* × *aa bb cc dd EE* will give a few individuals in F_2 (about one in five hundred) having the formula *AA BB CC DD EE* or *aa bb cc dd ee*, which are respectively taller than the tall parent and shorter than the short parent.

To analyse cases of continuous variation accurately measurements are usually necessary. The results can then

be plotted in the form of a graph. Suppose we wish to study height variation in a sample of human beings taken from the adult male population of Great Britain. If the measurements were made to the nearest inch, the results could be represented graphically by plotting a curve showing the number of individuals having each particular height. Figure 3 gives the results of measurements for 8,585 adult males, of which it will be seen that there were rather more than 100 individuals of 62 inches in height, about 300 of 63 inches, and so on ; by far the greatest numbers occurring in the neighbourhood of 67 or 68 inches, and very few being as short as 60 inches or as tall as 76.

In this case the curve is nearly symmetrical, and approximates to the one known as the normal curve of errors. This is a very common type of variation, though not the only one. It occurs when the value attained by an individual—whether in height or in some other character —depends upon the chance action of many independent circumstances. In the example given, the height of an adult male depended partly upon the particular set of hereditary factors he derived from his parents ; and partly upon a large number of unrelated circumstances, such as diet and illness, which influenced his development.

Another kind of distribution, the asymmetrical, is commonly found for variation in number of parts. An example, from Yule, is variation in petal-number in the buttercup, *Ranunculus bulbosus*.

Number of Petals.	Number of Individuals.
4	0
5	133
6	55
7	23
8	7
9	2
10	2

These results are shown graphically in figure 4.

Other types of curve are also possible, but we shall only take in detail cases approximating to the normal curve. This is likely to occur for the variation of a measurable

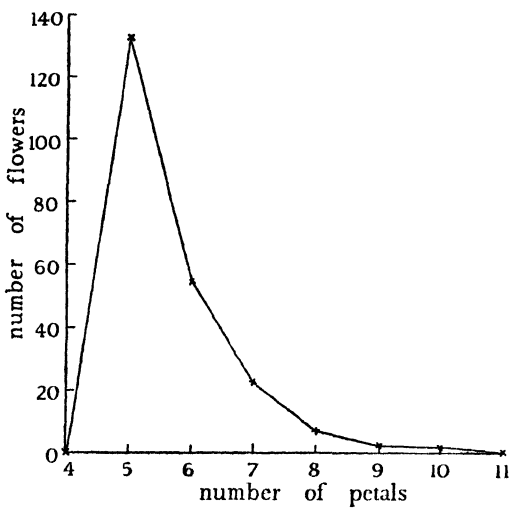

FIG. 4 (after Yule).

character within a pure line. An example, taken from Biffen's work on wheat, is given in figure 5*a*, which shows the variation in length of glume in two different pure lines of wheat; one having a mean glume length of about 11 mm., and the other one of about 30 mm., the average varying somewhat from season to season. The difference between the two lines is clearly defined—it is an example of discontinuous variation—but, as we might anticipate, when they are crossed, variation in F_2 appears to be continuous. The F_1 has a glume of intermediate length, and in F_2 the length varies continuously from about 11 to 35 mm. in the manner shown in figure 5*b*. The results from a

similar F_2, in which a different short-glumed parent was used, are represented in figure 5*c* (p. 58). In both cases we have an F_2 population, which is certainly not genetically pure for

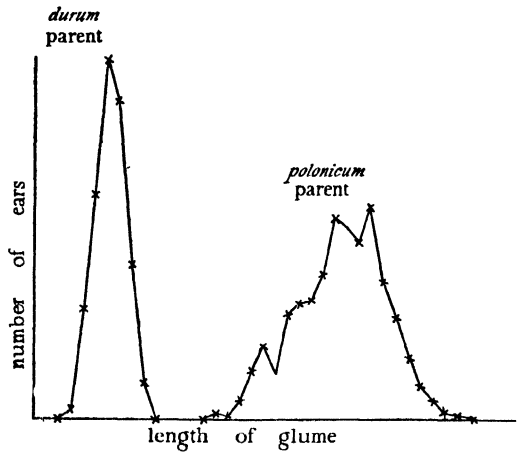

FIG. 5*a*.

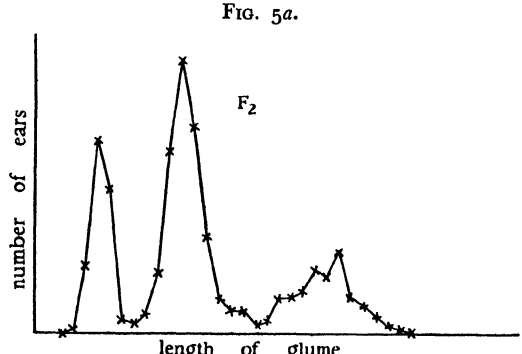

FIG. 5*b*.

glume length, giving continuous variation. In one case, that shown in figure 5*b*, the form of the curve, with its three well-marked peaks, suggests that the population may consist of more than one type; and in the other, given in figure

5*c*, the same conclusion suggests itself though less obviously. That these conclusions are correct has been shown by further breeding, which has demonstrated that each F_2 consists of three genetically different types : the two parental types, that is to say, long and short, and the intermediate heterozygote. Owing to fluctuation these three types overlap, the overlap being greater in some F_2*s* than in others ; but they are easily separated by further breeding, since the long type will give only longs in the next generation, while an

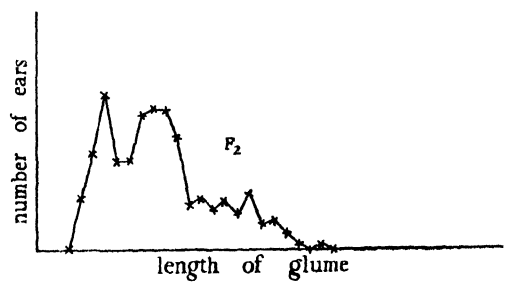

FIG. 5*c*.

intermediate which might be equally long will give the whole range of variation from short to long.

In figure 5*b* the three well-marked peaks suggested that there were three different types present ; but this is not so clear in figure 5*c*, the overlap being greater ; and it is evident that with a greater number of types and greater overlap the distribution in a population would take the form of the normal curve. In a population of animals or plants that is actually a mixture of genetically different types, variation may therefore appear to be continuous, and may easily take the form represented by the normal curve, just like variation caused by fluctuation only.

The effect of selection in a population of this kind is easy to see. In the case represented by figure 5*c*, selection of the most extreme individuals, whether long or short,

separates those which are genetically the same, that is to say, belong to the same *genotype*. It is only among the more intermediate values that individuals of different genotypes may have the same length, and selection would have no effect. If the population had consisted of bisexual animals, or cross-fertilized plants, the same result would have held so long as there were, as supposed, only three genotypes; since the extreme individuals would all be of the same type, as far as length is concerned, and would breed true.

When the number of genotypes is large, as it normally would be, the result of selection in cross-fertilized organisms is more complicated, and will be described in a later chapter. It is not difficult to see, however, that continuous selection of the most extreme individuals tends to isolate those that are most extreme genetically: selection for height would tend to isolate the tallest races. Furthermore, it may be anticipated that the mean value of the population will first change more or less rapidly; but that after a time, long or short according to circumstances, no further change will take place because the most extreme genotypes have already been selected.

Evidence supporting this conclusion is given by the results of selection in sugar-beet, which has now been carried on by plant breeders for some 100 years or more. This is equivalent to 50 generations, since the sugar-beet is a biennial. From 1838 to 1912 the sugar content of the roots was raised by selection from about 9 per cent. to about 18·5 per cent. Improvement was especially marked each time a better method was invented either for measuring the sugar content of the roots or for breeding technique.

During the last 20 years it is doubtful whether there has been much further improvement; and it is possible that the limit has been reached, though more might perhaps be done if a new and more effective technique of breeding

were discovered. It is interesting to notice that today it is said that selection still has to be practised to maintain the sugar content at its present level. Presumably this is largely because, the sugar-beet being a cross-fertilized plant, the different varieties are still heterozygous for factors affecting sugar content in spite of the long selection.

In *Drosophila melanogaster*, the fruit-fly, a selection experiment was carried out, in the U.S.A., by Payne for 60 generations. The character observed was the number of bristles on the scutellum. In 500 individuals of the original race the number varied from 0 to 3, with an average of 1·24; and selection was carried out for an increase in bristle number, in the so-called plus line, and for a decrease, in the minus line. Brother × sister mating was practised throughout.

On the whole the results of the experiment agree with Mendelian theory, though they showed one somewhat uncertain feature. In the plus line (fig. 6*a*) there was a rapid increase in bristle number for the first 20 generations, as might have been expected; and after this—apart from small changes, either up or down, which may fairly be attributed to fluctuation—there was no further change for about 25 generations. There seems to be a definite rise, however, from the 46th to the 55th generations; and the reason for this is not obvious. It is true that there was a fall again after the 55th generation, suggesting that the previous rise had no genetical significance, but reasons were given for believing that the fall was due to the conditions under which the experiment was carried out. There is therefore some evidence that a change of a genetical nature may have occurred at about the 46th generation. On the other hand the change in average bristle number involved is certainly very small compared with that brought about during the first 20 generations; and after this, as expected,

the number was almost constant for 25 generations, and showed little net change over the whole 40.

Towards the end of the experiment a back selection, for decreased bristle number, was made. A small effect could be seen at once; but for the next 12 generations there was appreciably no more. This means that the material was almost, but not quite, homozygous when the

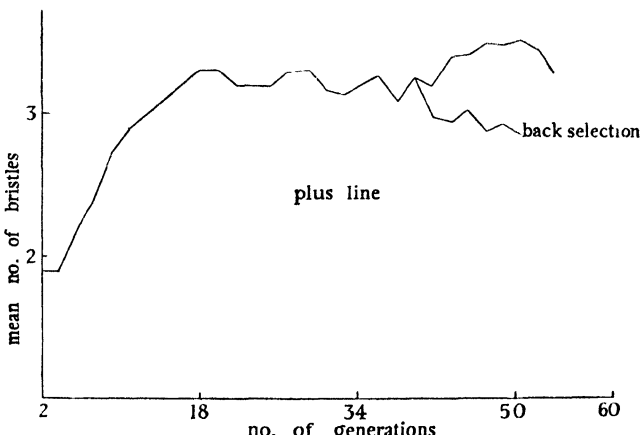

FIG. 6*a* (after Payne, in *Genetics*).

back selection started; and this conclusion agrees on the whole with what would be expected. For, brother × sister mating, though fairly effective in producing homozygous individuals, is less so than self-fertilization; so that even after 40 generations the material might not be quite homozygous, and back selection would isolate the individuals with the lowest average bristle number. The effect would not go far, however, because nearly all the factors for low bristle number would have been eliminated already.

In the minus line, selection was rapidly effective for 16 generations. Thereafter it had no effect, and the line

became stable with an average bristle number of 0·004. This means that although nearly all the flies had no bristles, in consequence of fluctuation a few had one or more, and it was impossible by selection to establish a line breeding true to no bristles. The results (fig. 6*b*) are best displayed by plotting a curve giving, in each generation, the percentage of flies with no bristles.

Except possibly for the doubtful case of the small rise in Payne's plus line of *Drosophila* at the 46th generation, the

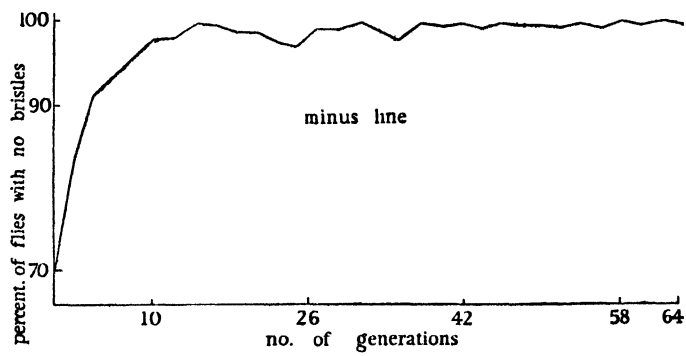

FIG. 6*b* (after Payne, in *Genetics*).

results of selection both in *Drosophila* and in the sugar-beet agree with the supposition that inheritance was Mendelian. There is no direct proof that the characters in question depend only upon Mendelian factors but the results agree with this supposition.

It should also be pointed out that Castle, in the U.S.A., carried out a long series of selection experiments in hooded rats, to prove that the size of the pigmented areas on the coats of these animals did not depend solely upon the action of Mendelian factors ; but the results obtained finally convinced him that a Mendelian explanation was the simplest.

In conclusion, Mendelian inheritance has been proved

so widely, and for so many different characters, that there is a presumption in favour of supposing it to be general. Known exceptions are rare, and appear to be special cases, as in the transmission of chloroplasts—the bodies containing the green colouring matter of plants—which are carried mechanically by the gametes.

The differences most suitable for genetical work are those that are large and easily distinguished, and these must give an exaggerated idea of the effect that the average Mendelian factor has.

At the same time, it would be possible to argue that heritable variations are of two kinds. The first are qualitative, often large and clear cut, like the difference between red and yellow flower colour, which would probably be caused by two distinct chemical substances. It will readily be agreed that these are due to Mendelian factors. The second kind is quantitative—variation in the degree to which a character develops and not in its nature. In this case the differences involved are often small and may vary continuously; and the question has arisen whether they also are controlled by Mendelian factors.

The argument against this supposition is not strong. It rests solely upon the fact that cases of continuous variation are usually difficult to subject to rigid Mendelian analysis because the fluctuations due to environmental conditions are larger than the variations caused by heredity.

Evidence in favour of a Mendelian interpretation is of several kinds. In the first place there is presumptive evidence that continuous variation is due to the action of many independent factors; and the results of selection experiments are consistent with this view. The pure-line theory provides the second, and perhaps the most important, line of evidence. Ultimately, Mendelism demands only that heredity is due to the transmission of constant discontinuous

differences ; and this is exactly what the pure-line theory demonstrates, though complete proof is lacking since it is possible that the factors responsible for the differences between pure lines might sometimes break up when crossing occurs, and might under some circumstances recombine. Most of this book, however, is written on the assumption that this is not the case. Finally, factors that influence the degree to which a character develops, known as *modifying factors*, have been proved to exist. The demonstration depends upon a special genetical phenomenon to be described in a later chapter.

Considering the special difficulties attending the analysis of continuous variation the evidence that it has a Mendelian basis is undoubtedly strong.

The analysis of variation here carried out suggests important conclusions. In the first place, a clear distinction has been drawn between variations that are inherited and fluctuations caused by the environment, which are not. Darwin was inclined to assume that any difference was inherited, but this is definitely wrong : part of the variation normally observed is fluctuation, like the variation within a pure line, and is not transmitted to the offspring.

Secondly, selectionists have often tended to assume that the powers of selection are unlimited, though it was pointed out long ago to the more ardent exponents that the size of a sheep's head may be reduced by selection but cannot be reduced indefinitely. It is now clear that selection can do no more than isolate the best combination of existing factors, though we cannot tell beforehand how much better that best will be.

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CHAPTER V

CELL DIVISION

IN describing Mendel's theory of inheritance, certain assumptions were made about the physical nature of reproduction : for example, that sexual reproduction consists of the union of two gametes, an egg-cell derived from the female parent and a sperm derived from the male. It was also tacitly supposed, and we have seen that results justified the assumption, that these two cells make an equal contribution to the zygote. Still more important, perhaps, it was assumed that when the germ cells are formed there occurs a process described as segregation, by which the two members of each pair of elements or factors making up the zygote are distributed to different gametes.

Since Mendel's day, when little was known about the subject, all these assumptions have been verified by microscopical study, and the science of cytology has contributed as much as Mendelism to our knowledge of heredity.

In 1665, soon after the discovery of the microscope, Hooke observed that a piece of cork had a cellular, or honeycomb, structure ; and later observations showed that this structure is characteristic of most forms of life. What he saw, however, was merely the dead walls formed by the living elements within and really the least important parts of the cells.

It was not until after 1838 that the real importance of cells was realized. In this year Schleiden and Schwann

concluded that every living organism consists entirely of cells. Many of the other views of these authors were erroneous; but this particular conclusion led to general recognition that cells are the structural and functional units of which plants and animals are made—a conception which is one of the most important in the history of biology.

A further important step was made about 1844, when Nägeli and later writers showed that cells only arise from the division of other, already existing, cells.

In their most fundamental structure all cells are alike. All contain a living substance called protoplasm, which is made up of a nucleus, usually spherical, and the more fluid cytoplasm in which the nucleus is imbedded (fig. 7*a*, p. 69). But in size and appearance cells vary very much. The largest are the "yolks" of the unfertilized eggs of birds and reptiles, which are technically single cells, though the enormous bulk is made up almost entirely of reserve food, upon which the minute product of fertilization will grow rapidly until it hatches. Usually, however, cells are microscopic in size. Though they vary greatly, a common size would be that of a cube having sides about 20 microns in length—a micron equals one-thousandth of a millimetre, and the pages of this book are about 220 microns in thickness. In shape and consistency they are as variable as they are in size, and it is these different kinds of variation that make differentiation possible; that is to say, the production of different kinds of cell, such as nerve-cells and muscle-fibres in animals, or woody vessels and pith-cells in plants. Young cells are not differentiated at all. In the growing point of a root-tip and the early embryonic cells of many animals, all the cells look alike, and except for possible size differences there is nothing to distinguish one species from another; indeed, but for the fact that the cells of plants are nearly always surrounded by a wall and the cells of animals have

none, undifferentiated cells from the two kingdoms might often be indistinguishable.

It is clear that in the last resort heredity, indeed the whole chain of life, must be the consequence of cell division. Cells only arise from the division of a parent cell ; and every individual comes from the divisions of the fertilized egg—the product of union between an egg-cell, or female gamete, and a sperm, or male gamete. This single cell divides into two, the two daughter cells divide again, and so on. The cells produced by these repeated divisions grow variously in shape and size, become arranged in different ways, and so give rise to organisms with characteristic features.

The exact nature of the process effecting division is a matter of fundamental importance to genetics. It can be studied in living material ; but the observations are difficult to make because both nucleus and cytoplasm are transparent, and it is therefore usual to treat them with a coloured stain to make them easier to observe. To do this properly the living cell must first be treated with a fluid, the fixative, which kills it and preserves the structure of both nucleus and cytoplasm as closely as possible to that of the living state. In recent years the methods for doing this have been greatly improved, and in favourable circumstances the grosser structures are practically unaltered by treatment. This is shown clearly by plate I, which shows photographs of the same cells taken alive and after treatment with fixative and stain. In the finer details, of course, fixed material differs from living material because the proteins are coagulated ; just as the ultimate structure of egg albumen is altered when the egg is boiled. Fortunately it is the grosser structures that have had the greatest interest for the genetical worker up to the present.

In a young, undifferentiated cell the cytoplasm appears to be homogeneous. The nucleus, which is the most im-

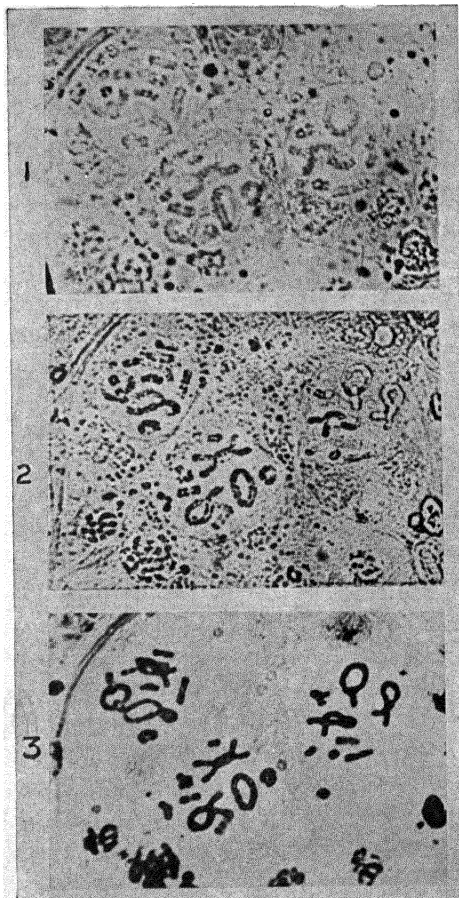


PLATE I.

Chromosomes in *Stenobothrus lineatus* (from Belar).

Fig. 1. Living cells. Fig. 2. The same cells after fixation. Fig. 3. The same cells after fixation and staining.

portant part, is globular in shape ; it is denser and more viscous than the cytoplasm, and so long as the membrane enclosing it is not torn it can be pushed about with a needle and may be moderately indented without injury. It has no visible structure, because, being more or less transparent and not very different from the surrounding cytoplasm, it is not easily observed when living. Other observations show that it cannot really be homogeneous either physically or chemically. When fixed and stained it usually appears as a fine network lying in a transparent

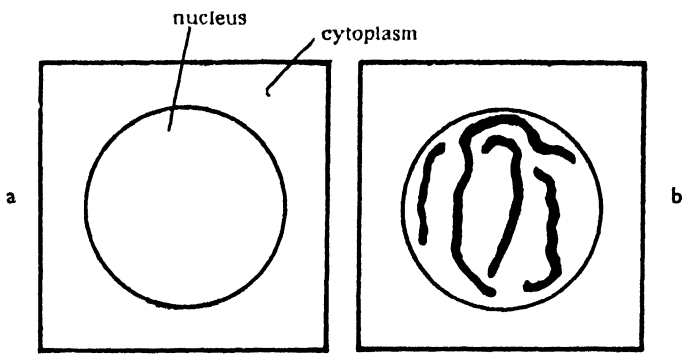


FIG. 7.—Diagram of somatic cell division or mitosis in cell with four chromosomes.
a, Cell before division; *b*, prophase.

matrix ; and it may contain one or more spherical bodies, the nucleoli, but these disappear during division and are not believed to have any genetical importance.

Division of the nucleus, *mitosis* or *mitotic division*, is far from a simple process. Briefly, it first resolves itself into a number of rod-shaped bodies, the *chromosomes* ; each of these splits longitudinally ; the halves separate into two groups, and a new nucleus is formed from each group.

The chromosomes cannot be seen in the nucleus until the cell is about to divide—the stage called *prophase*—when

they appear as slightly denser, thread-like bodies (fig. 7*b*). Their gradual formation can only be followed in fixed material. After they are formed the nuclear membrane suddenly disappears, setting free the chromosomes, which

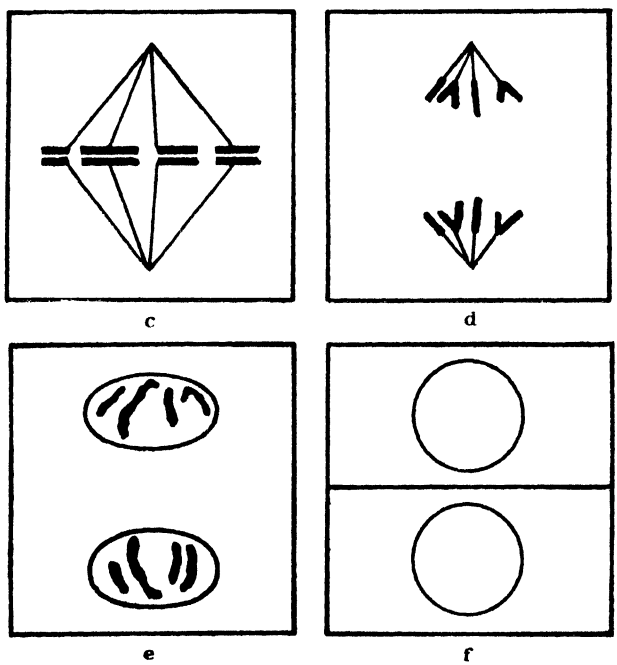


FIG. 7.—Diagram of somatic cell division or mitosis in cell with four chromosomes.
c, metaphase; *d*, anaphase; *e*, telophase; *f*, completion of division.

arrange themselves in one plane across the centre of the cell, and remain there for perhaps fifteen minutes in active writhing movement (fig. 7*c*). At this stage, *metaphase*, the chromosomes can be studied in detail in fixed material. At the same stage it can be seen that a *spindle* has developed. This has the appearance of a number of fine threads, the

spindle fibres, stretching from the chromosomes to two points at opposite poles of the cell, though the exact nature of the spindle is a matter of doubt ; in living cells a needle can be passed across it without seeming to produce any effect, so that to speak, as is customary, of threads or fibres is somewhat misleading. The chromosomes are already split longitudinally at this stage, and the two halves are only held together at the point on the chromosome at which the spindle fibre is attached, the *attachment constriction* as it is called. Finally, the halves of the chromosomes suddenly separate from one another and pass to opposite

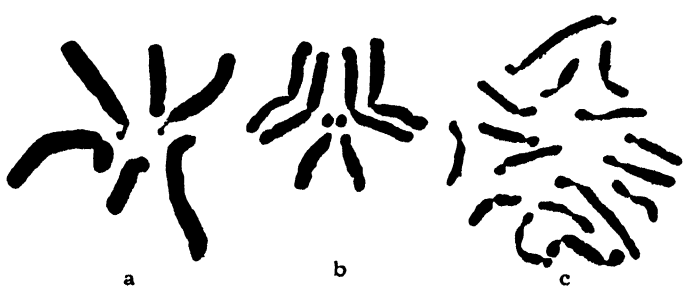


FIG. 8.

Chromosomes of : *a*, *Crepis capillaris*, 3 pairs (after Navashin) × 2,500 ; *b*, *Drosophila melanogaster*, 4 pairs (after Bridges) × 9,000 , *c*, *Crocus hadriaticus* (after Mathei) × 2,900.

poles of the cell (fig. 7*d*) where the two daughter nuclei are formed (fig. 7*e*). Division of the cell is completed by division of the cytoplasm, which is effected in plants by the laying down of a wall and in animals by furrowing without wall-formation.

At metaphase the chromosomes can usually be counted ; and it has been found that in any individual the number of them that appear at division is constant, and is usually the same in all the individuals of one species. In man it is 48 ; in the rat, *Rattus norvegicus*, 42 ; in the fruit-fly, *Drosophila melanogaster*, 8 ; and in the sweet pea, 14. Some-

times, as in roses, *Rosa*, or wheat, *Triticum*, all the chromosomes look alike ; but very often, as in *Drosophila*, some of them are recognizably different from the others (fig. 8), and in these cases it will be noticed that they occur in pairs (fig. 8*a*, and *b*).

Variation may occur in size, in the position of the spindle-fibre attachment which may divide the chromosome into two equal or nearly equal arms, or may be terminal or nearly terminal, and sometimes in the possession of other constrictions. These differences between chromosomes appear at every cell division, and the same differences are found in other individuals belonging to the same species. Thus, although the chromosomes disappear from view at the end of each division, they seem to reappear unchanged at prophase ; and it is now generally believed that they are permanent structures which remain the same from one division to the next. They certainly behave as if this were the case.

The remarkable feature of cell division is that although the cytoplasm divides simply by fission, the nucleus divides by a decidedly roundabout and complicated process. First, there is the appearance of the chromosomes, which appear to be its constituent elements, then each of these divides longitudinally and the halves pass to opposite poles, so that each daughter nucleus receives an identical share. This has suggested that the nucleus, and especially the chromosomes, have important parts to play in hereditary transmission. Indeed, as long ago as 1883, it was pointed out by the German biologist Roux that the process of nuclear division would be meaningless unless this were the case, and unless the chromosomes were differentiated along their length.

This conclusion has been amply confirmed. In the first place, at fertilization both egg-cell and male gamete contain a single nucleus, but whereas the mass of the cytoplasm

in the former is very large in the latter it is often almost negligible. It was long ago argued, therefore, that if the cytoplasm played any part in inheritance, the F_1 between two types would usually resemble the mother rather than the father. This is not the case, however. *Reciprocal hybrids*, that is to say hybrids made first with one form as the mother and then with the other, are identical in appearance, with one or two rare exceptions. In some of these rare cases it seems likely that there may be a difference in the cytoplasm of the two types crossed; apart from these it is highly probable that the differences are carried by the nuclei, and of these the only permanent structures appear to be the chromosomes.

Evidence of this kind was amplified by the German zoologist Boveri, towards the close of the last century. He carried out a number of ingenious experiments to demonstrate the importance of the chromosomes in hereditary transmission. Thus he was able to show that if any chromosome were lacking in the zygote, then the embryo did not develop normally. His experiments were of great weight, and went far towards proving the thesis in question; but since the rediscovery of Mendelism other evidence of an equally, if not more, cogent nature has become a commonplace of genetical research. This will be described on subsequent pages.

It will be realized that if the chromosomes are constant units, transmitted unchanged from one cell to the next, then, since two cells unite at fertilization to give one, from which the new zygote is derived, there must be some stage in the life-cycle at which the number of chromosomes in the cell is reduced to half the usual number. Otherwise the number would be doubled at every new generation.

This reasoning led the German biologist Weismann, in 1887, to predict the existence of a reducing division, which

was found later to occur, in place of a mitosis, shortly before the formation of the gametes. Thus, an organism with 6 chromosomes in the body-cells forms gametes with 3 ; and the union of two gametes at fertilization restores the original number. The divisions at which this reduction in chromosome number occurs are known as the *reduction divisions*, or *meiosis*.

The general principle of meiosis is that the chromosomes, instead of splitting longitudinally into two halves which pass to opposite poles, associate together in pairs, each chromosome with another similar chromosome. The two members of each pair then pass to opposite poles of the cell, where two nuclei containing half the usual number of chromosomes are formed. This completes the first of the two reduction divisions, the *heterotype* division. It is followed at once by a second division, known as the *homotype* division, which is exactly like an ordinary mitosis. As a result of the two divisions four cells are produced, each containing the reduced number of chromosomes ; and from these cells the gametes are formed.

The pairing of the chromosomes at meiosis begins in the early stages, the prophase, when the chromosomes are in the form of long thin threads. As pairing is completed the chromosomes contract, becoming ultimately short and thick, and pass on to the equatorial plate to give metaphase (fig. 9*b*). It has already been mentioned that in some organisms individual chromosomes can be recognized, and in these cases it is found that like chromosomes pair at meiosis ; *A* with a similar chromosome *a*, *B* with *b*, and so on. Each pair of chromosomes is said to be a *bivalent* ; and the two members of a pair are said to be *homologous*. In many cases, of course, it is impossible to recognize individual chromosomes ; but it is assumed—and we shall see that the assumption has been amply justified—that though the

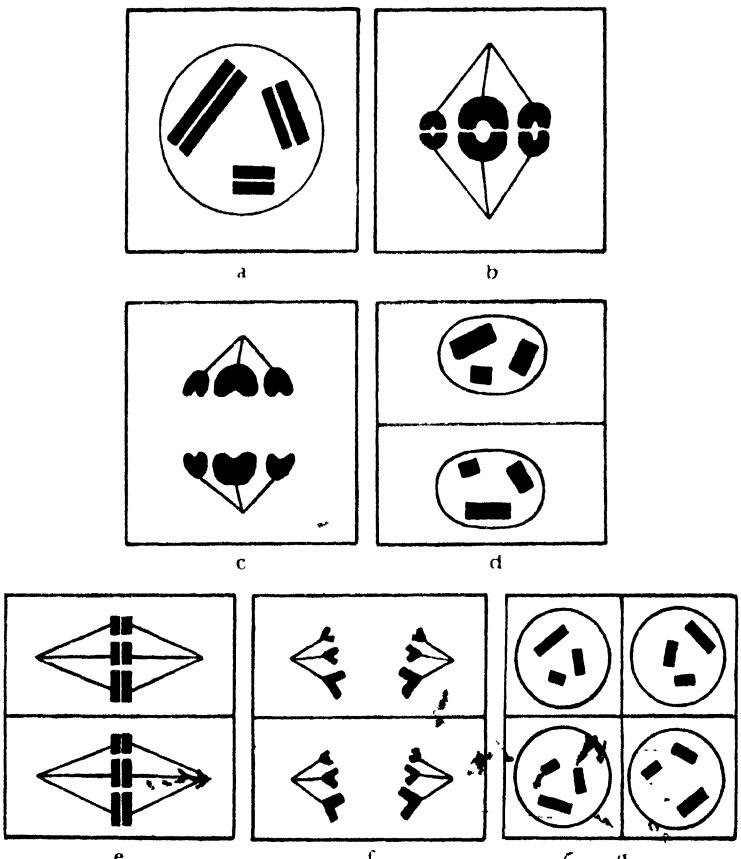


FIG. 9.—Reduction divisions, or meiosis, with six chromosomes, diagrammatic.

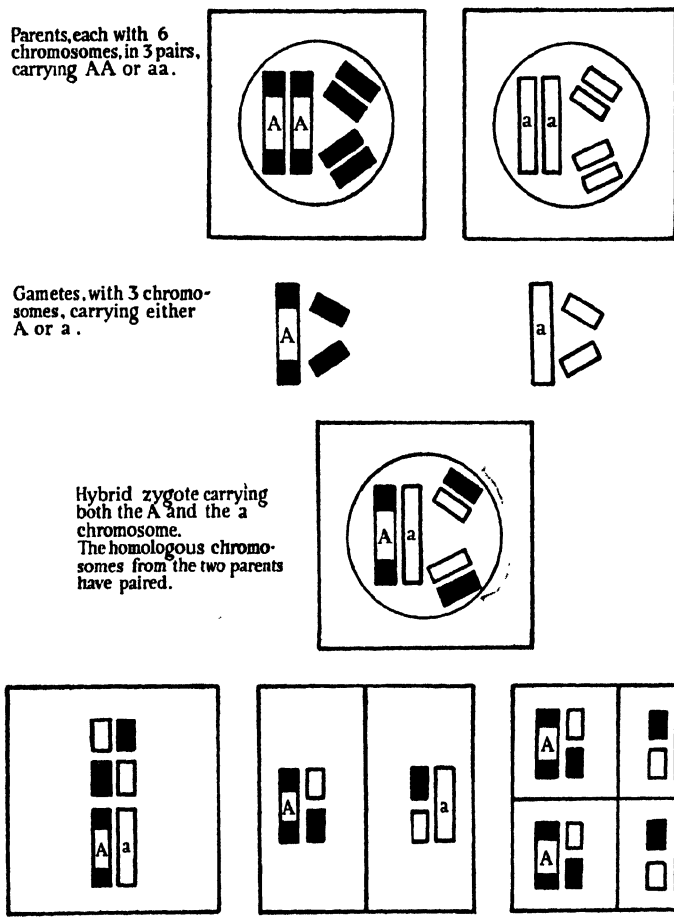
a–d, Heterotype division; *a*, during prophase the 6 chromosomes have formed 3 pairs. *b*, metaphase, with 3 bivalents; *c*, anaphase, the paired chromosomes have separated and 3 are passing to each pole; *d*, the re-formed nuclei; *e–g*, homotype division; *e*, metaphase, each cell with 3 longitudinally split chromosomes; *f*, anaphase, the halves are passing to opposite poles; *g*, 4 nuclei, each with the reduced number of chromosomes, result.

chromosomes look the same they differ in their composition and genetical properties, and that only those that are alike pair, just as they can be seen to do when they are visibly different. In an organism with 6 chromosomes there will, therefore, be three pairs at metaphase. After a short interval, the two members of each pair pass to opposite poles of the cell, *anaphase* (fig. 9*c*), and form two daughter nuclei, *telophase*, each with three chromosomes, thus completing the division (fig. 9*d*). Immediately after the heterotype division, often before the two nuclei have entered a proper resting-stage, comes the second, or homotype, division. The reduced number of chromosomes pass to the equatorial plate and split longitudinally (fig. 9*e*), and the halves pass to opposite poles (fig. 9*f*) where the daughter nuclei are formed (fig. 9*g*). The two divisions therefore give four cells, each with the reduced number of chromosomes ; and these cells give rise to the gametes, either directly, or after further divisions.

In the male, as a general rule, each of the four cells will form a spermatid, in animals, or a pollen grain, in plants. In the female only one of the four will form the female gamete, the egg-cell, and the other three degenerate.

The union of two gametes at fertilization restores the full somatic number of chromosomes. Often, the reduced or gametic number of chromosomes is referred to as the *haploid* number and is denoted by n, and the double or somatic number is called the *diploid* number, denoted by $2n$; but the exact use of the terms haploid and diploid must be explained more fully on a later page.

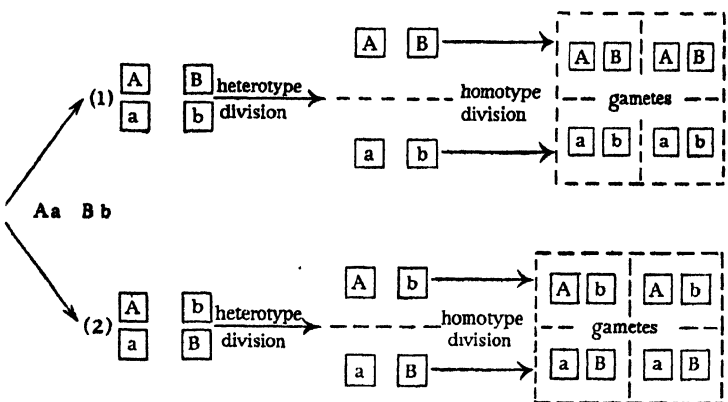
If it be correct to assume, as we have done, that at heterotype prophase and metaphase the chromosomes do not pair haphazardly but always the same two pair, *A* and *a* or *B* and *b*, then the heterotype division is exactly the mechanism postulated by Mendelian segregation (fig. 10). If the chromosomes carry the Mendelian factors, and an



Reduction divisions in hybrid, giving 2 germ cells with A and 2 with a

FIG. 10.—Segregation.

allelomorphic pair such as *A* and *a* are carried by two homologous chromosomes which pair at metaphase, then the gametes produced will carry either *A* or *a* in the proportion 1*A* : 1*a*. The mechanism would also permit the independent segregation of factors, and consequent recombination, found by Mendel. In a hybrid such as *Aa Bb*, heterozygous for two pairs of factors, if *A* and *a* are carried by one pair of chromosomes, and *B* and *b* by another pair, it will be a matter of chance whether *A* is



carried to the same pole as *B* or the same pole as *b*, and independent segregation results.

It is now generally agreed that this theory is correct; that is to say, the Mendelian factors are carried by the chromosomes. The nature of some of the evidence supporting the theory will be indicated in this chapter, though it need not be dealt with in detail since the theory forms the basis of modern genctical work, and abundant evidence in its favour will therefore be found in pages that follow.

One of the principal assumptions involved is the individuality of the chromosomes. It is assumed that although the chromosomes disappear from view in the period between

one division and the next they do not lose their individuality, the same chromosomes reappear at every division. In its essentials, this assumption has proved to be fully justified. The strongest support for it comes from hybrids between forms in which the chromosomes differ either in number or in morphology. In these cases the different chromosomes can be recognized in the hybrid and traced in succeeding generations; and it is plain that, whatever physical and chemical changes they may undergo between one division and the next, they reappear at each division essentially unchanged.

If it be granted that in general the chromosomes are permanent in their essential structure, the belief that they bring about the segregation of the Mendelian factors is upheld principally by the parallelism found between factor segregation and chromosome segregation. This parallelism is notable enough in the case already described—the regular heterotype division. It is still more notable in the numerous examples now well known in which the chromosomes behave in an irregular way at reduction, and the segregation of the Mendelian factors follows an exactly similar irregularity.

The study of cell division has therefore greatly advanced our understanding of the hereditary process. In somatic divisions the nucleus of the cell first becomes visibly resolved into the chromosomes, which are usually constant in number for every individual and for the species; the chromosomes are then accurately divided, by longitudinal splitting, between the two daughter nuclei. This scrupulous division itself suggested that the chromosomes might be important in heredity; the suggestion has been confirmed by their behaviour at meiosis, which strongly suggests that they bring about the segregation of the Mendelian factors.

If these conclusions are correct, and the argument is reversed, then the seemingly exact division of the chromo-

somes which occurs at somatic divisions suggests that the daughter nuclei so formed are exactly alike; that is to say, all the body-cells of an individual are genetically identical, and would all give a like result if propagated, despite the fact that they differ greatly in appearance.

It is worth while inquiring a little more deeply how far this conclusion is true; and if it is correct, to ask how differentiation is effected. If all the cells of the body are alike genetically, how have they come to be so different in appearance? How have identical cells formed such different products as nerves and muscle?

In the first place, it will be recalled that an example has been given already, awn development in wheat, in which different parts of the same plant were proved to be identical genetically, though they differed in appearance; for the progeny were the same whether they were descended from the awned early tillers or the awnless late tillers. In the ordinary way the germ-cells of the higher plants are descended from the cells of the sub-epidermal layer, the one next to the outermost layer, so that breeding only tests the genetical composition of this layer, and we cannot show that the other layers of the stem, the roots, or elsewhere, are genetically all the same though so different in appearance. The experience of vegetative reproduction, however, does strongly support the conclusion that different parts of the same individual are genetically identical.

Thus a whole plant will develop from a cutting taken from a small portion of the stem. Similarly, many plants may be propagated from a portion of the root, some even from a few cells of the leaf; and if a young plant is decapitated and a callus, or wound-tissue, forms at the cut surface, buds may form anywhere on the callus and develop into branches showing no difference from normal shoots. Many varieties of cultivated plants, especially fruits, are

always propagated vegetatively and perfectly retain their constancy. All the trees of Cox's Orange Pippin in the world today, and those that have existed at any time in the past, are parts of a single individual that was first produced one hundred years ago. These trees are all identical, except for small variations in productivity, determined by the stock on which they are grafted. Again, so far as can be told from the illustrations in Gerard's Herbal, the *Tulipa Clusiana* of today, propagated by bulbs, is exactly the same as it was over three hundred years ago when the Herbal was published.

To some extent, evidence of a similar kind is given by animals. In *Planaria*, flat-worms that mostly live in streams, almost any piece cut off will regenerate the whole. In the salamander, if the two cells formed by the division of the fertilized egg are separated, each will develop into a perfect embryo of half size, showing that the two cells are genetically identical ; and in parasitic wasps, or ichneumons, which lay their eggs in the bodies of caterpillars, during development the cells fall apart into groups, each one of which gives a complete larva.

In these cases it seems clear that cell division has not produced any genetical differences, and the reason that the different cells of the two- or eight-celled embryo normally develop into different parts of the animal must be that their development is affected by their position in relation to neighbouring cells.

In other cases we may be forced to a rather different conclusion. Thus, as shown by Strangeways at Cambridge, if cartilage cells of the chick are grown in culture, though differentiated cells like this are normally past division, they can be made to become more and more like young undifferentiated cells in appearance, until finally they become quite de-differentiated and will undergo division. They

may then be indistinguishable from de-differentiated cells of some other tissue, but if allowed to differentiate again they only form cartilage and will not form tissue of a different kind.

In this case—and the same conclusion is given by some cases of regeneration and other work on the causes of differentiation—it seems that differentiation has been accompanied by some permanent alteration in the nature of the cell. The nature of this alteration, and how it has been brought about, are not clearly understood.

On the whole, however, the numerous cases where differentiation has been shown to occur without any genetical change support the conclusion that equal division of the chromosomes does give genetically identical products, whatever may be the nature of the changes responsible for differentiation. This is especially clear in plants, where differentiation and specialization are not carried as far as in animals.

(*References will be found at the end of Chapter VI.*)

CHAPTER VI

SEX DETERMINATION

WE have seen how the normal pairing and segregation of the chromosomes in the heterotype division is exactly like the segregation of factors in simple Mendelian inheritance, suggesting that the chromosomes bring about the segregation of the factors. The proof of this has become much stronger because it has been shown that when the chromosomes behave unusually then factor segregation shows an exactly similar departure from the normal. Many examples of this have been worked out; indeed, it is now the universal practice, when genetical segregation appears to be following an unexpected course, to try to understand the situation by observing the behaviour of the chromosomes.

One of the first cases to be studied was the determination of sex, and the problem of sex-linked inheritance, in bisexual animals. In the simplest examples the connexion between chromosomes and sex is quite clear, but difficulties were soon brought up by critics of the theory, and the work that has had to be done in solving the more difficult cases has brought a great increase in knowledge of genetical principles.

At the close of the nineteenth century it was known that in certain insects an anomalous body could be seen during the reduction divisions, which had in some ways the appearance of a chromosome, yet behaved quite differently, since it lagged behind the chromosomes in the heterotype division

and subsequently passed undivided to one pole of the cell. That this body was a true chromosome, concerned with sex-determination, was first suggested by McClung; and the theory was firmly founded in the earlier years of the present century by Wilson, in the U.S.A., and others. It has now been shown in a wide range of species that the female has two similar chromosomes *XX*, and the male two unlike chromosomes *XY*, one of them like those of the female, and the other, *Y*, either different from *X* or absent altogether.

When, on an earlier page, the reduction division was described, it was stated that the chromosomes associate in pairs; with the consequence that all gametes contain the same number of chromosomes, and that the zygote contains an even number, two of each kind. In the simplest case of sex determination, for example in certain of the Hemiptera (plant bugs, etc.), this is not true. In these the number of chromosomes in the male is odd and one less than in the female. If the female has 24 somatic chromosomes ($2n = 24$) which unite at meiosis to give 12 bivalents ($n = 12$), then the male has only 23 chromosomes ($2n = 23$), giving at meiosis 11 bivalents and 1 univalent, or unpaired, chromosome. It was supposed that the 11 pairs of chromosomes are not specially concerned with sex. These are usually known as the *autosomes*. And that 1 pair, the so-called *sex chromosomes* or *X* chromosomes, is specially concerned with sex. The female of such a species may be designated *XX*; and the male, with only one sex chromosome, *XO* or simply *X*, the *Y* chromosome being absent.

At the reduction divisions the behaviour of the *X* chromosomes in the female is like that of the autosomes. The two *X* chromosomes pair, pass to opposite poles at heterotype anaphase, and divide in the usual way at the homotype (fig. 11), so that every gamete has a single *X* chromosome

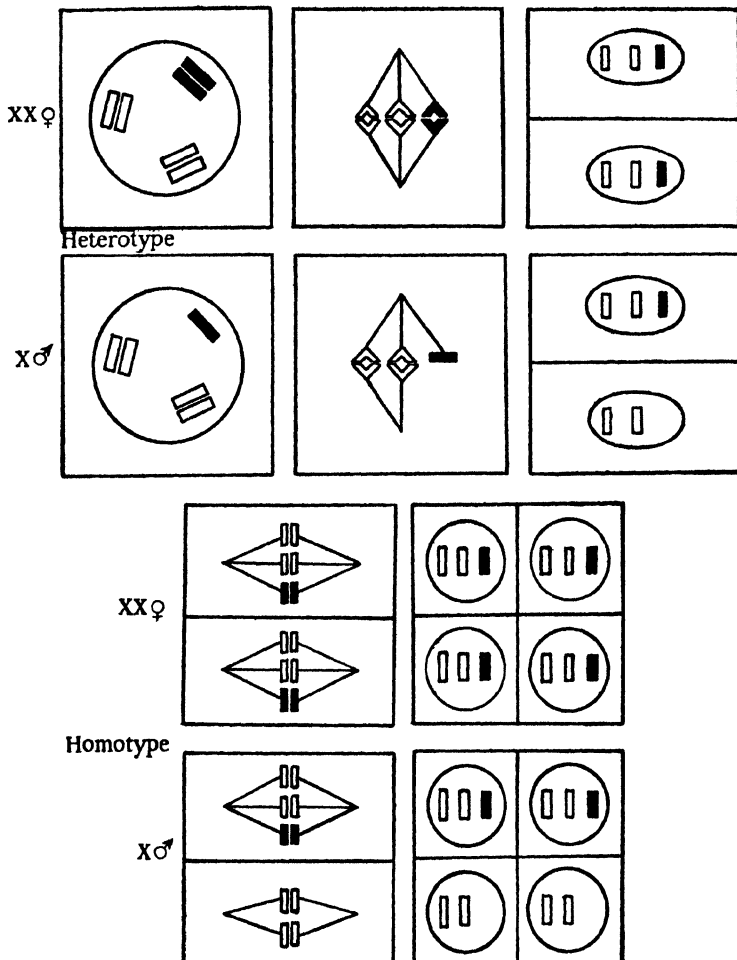


FIG. 11.—Reduction divisions in XX♀ and X♂ showing behaviour of unpaired X chromosome in ♂.

just as it has a single representative of each of the 11 pairs of autosomes.

In the male, also, there are 11 pairs of autosomes which behave quite normally; the single *X*, having no mate, behaves in another way. Its behaviour varies in detail in different species, but most frequently it passes to one pole of the heterotype division without dividing so that one daughter nucleus contains an *X* and the other is without it. At the homotype division it will be present in half the cells and absent from the other half. When present it divides equationally and the halves pass to opposite poles, just as the autosomes do. As a result of the two divisions there will, therefore, be formed two cells containing an *X* and two without it (fig. 11, p. 85).

This is the commonest type of behaviour for an unpaired *X* chromosome. In other species it may behave somewhat differently, but in any case the net result is the same: gametes are formed either with the *X* or without, and the two classes are formed in equal numbers.

To conclude, the females, *XX*, produce egg-cells that are all alike in containing a single *X*. The males, *X*, on the other hand, produce two kinds of sperm in equal numbers, those with *X* and those without. Random mating between egg-cells and sperm will evidently give females, *XX*, and males, *X*, in equal numbers.

Here we have an obvious case in which sex may be determined by the chromosomes. Against accepting such a theory universally there at once arises the difficulty that in some species of animal there is no apparent difference between the chromosomes of the two sexes.

This difficulty was overcome by the researches of Wilson. He showed that in some species of Hemiptera the female had two large *X* chromosomes and, corresponding to these, the male had one large one like those in the female and

one small one. This chromosome in the male varied in size in different species, and a complete series could be traced between forms in which the male had a single *X*, through those in which the *X* had a mate of varying sizes, to those in which the male, like the female, has two sex chromosomes which are apparently alike. The conclusion

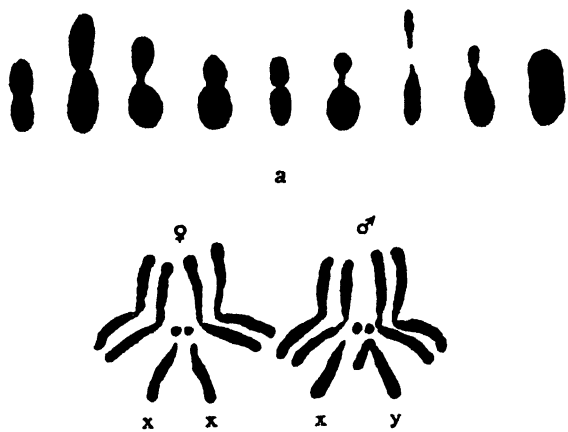


FIG. 12.

a, X (below) and Y chromosomes paired end to end, and showing transition from equality in size between X and Y to absence of Y (after Wilson and Stevens); *b*, chromosomes in male and female *Drosophila melanogaster* (after Bridges).

which is now generally accepted may be expressed by saying that one sex has two sex chromosomes, *XX*, and the other has one *X* chromosome and one *Y*. The *Y* does not always take the same form but is always different from the *X*. Sometimes it is distinguishable by its size or shape. Sometimes it is not present at all. In other cases it looks like the *X* but is presumed to differ from it in genetical properties (fig. 12).

In most bisexual animals the male is the heterozygous sex, *XY*, and the female is homozygous, *XX*. In the Lepidoptera, birds, and some fishes, the female is the heterozygous sex, *ZW*, and the male the homozygous, *ZZ*.

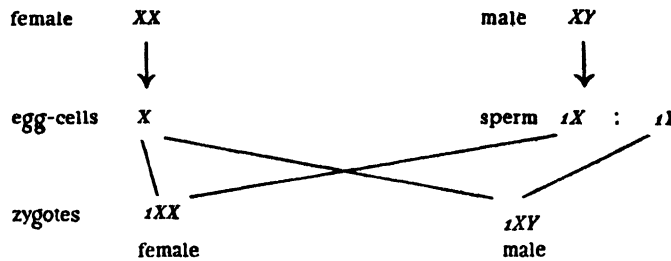
In most plants, male and female organs—stamens and ovaries in flowering plants—are both borne by the same individual, and the problem does not arise. In some species, however, the sexes are separated; and, among these, sex chromosomes have been established in the moss *Sphaerocarpos*, and in the genera *Melandrium*, *Humulus* (hops), *Rumex* (docks and sorrels), and *Fragaria* (strawberry) among flowering plants.

The evidence described strongly suggests an association between a particular pair of chromosomes and the sex character. It may be suspected—rightly as it proves—that a chromosome will affect more than one character; and the sex chromosomes do indeed affect other characters besides sex, a special type of inheritance known as sex linked being the result.

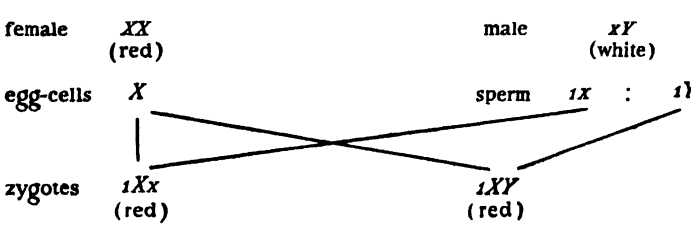
Mating in bisexual animals, *XY* and *XX*, resembles so far as the sex chromosomes are concerned a back cross between an F_1, *Aa*, and one of its parents, *AA*. The heterozygous sex, *XY*, is always crossed to the homozygous sex, *XX*; neither heterozygote nor homozygote is selfed. The female gives egg-cells all of one kind, *X*. The male gives two kinds of sperm, *X* and *Y*, in equal numbers. Random mating would then give male and female zygotes in equal numbers. The sex ratio, therefore, should normally be 1 : 1; and this is often the case, but it can be disturbed more or less seriously by several factors to be mentioned later.

It will be noticed that, in the following diagram, the *X* chromosome is transmitted in a rather curious way. The

ıale always transmits his *X* to his daughters ; he never ·ansmits it to his sons, who receive their *X* from their mother.

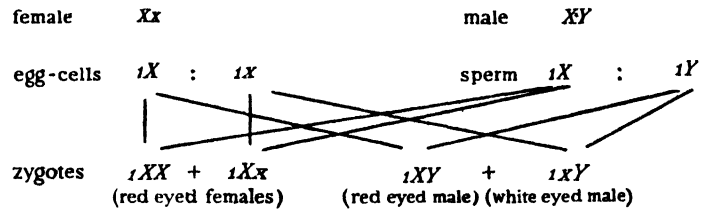


This gives sex-linked inheritance a character of its own, and numerous examples are known in different species of animal, including man. Some of the best-known cases occur in the fruit-fly, *Drosophila melanogaster*, and the inheritance of white eye-colour in this species will be taken as an example. The factor concerned is associated with the *X* chromosome, and a red-eyed male may be denoted *XY*, and a white-eyed male *xY*. Mating a white-eyed male with a red-eyed female gives only red-eyed offspring, since red is dominant.



The daughters from this mating, however, being heterozygous for white eyes, will transmit the character to half their sons ; so that, mated with a normal male, they give

red-eyed and white-eyed sons in equal proportions but only red-eyed daughters.



Other matings give different results, which are easily worked out. Thus a white-eyed female can be produced by mating a white-eyed male with a female carrying the white-eyed character.

Again, in man, colour blindness is a sex-linked recessive found only rarely in females ; and it is not difficult to see that this is because it would only appear in a female when a colour-blind male mates with a female carrying the character ; normally it is found in males and is transmitted by them, through their daughters, to half their daughters' sons.

Examples of sex-linked inheritance have been found in many animals. In Lepidoptera, and other cases where the female is the heterozygous sex, the same principle holds good, though the relative positions of the two sexes are reversed. Sex-linked inheritance makes it possible to predict a sex-chromosome mechanism in species not examined cytologically. It has also been applied practically in poultry management. In normal breeds of poultry the two sexes can only be distinguished when they assume adult plumage, but by a proper choice of parents they can be distinguished at hatching. This is achieved by using a dominant sex-linked character, barred plumage. Normal cocks—the male being the homozygous sex in poultry—may be denoted by ZZ, the normal hens by ZW and a sex chromosome that gives barred plumage by Z' instead of Z. It will then be

clear that normal cocks, ZZ, mated to barred hens, $Z'W$, give two classes of offspring : barred cocks, $Z'Z$, and normal hens, ZW, which can be separated at hatching.

Sex-linked inheritance is a simple example of the fact that when chromosome transmission follows a special course then the transmission of factors also follows that course. Other cases of identity between chromosome behaviour and factor segregation are now well known ; one of the earliest and most convincing, described by Bridges, being one in which the X chromosome itself behaved irregularly during meiosis and there was a corresponding irregularity in the inheritance of a sex-linked character. These examples, however, although they help to establish that the chromosomes are responsible for hereditary transmission, still leave much to be discovered about the way characters—whether sex or any other attribute—are determined. Thus, it has been shown that in some species an individual is female when it has two X chromosomes and male when it has one ; but it is natural to ask why a mere alteration in the number of X chromosomes present should bring about such a difference.

This problem is made clearer by some observations of Bridges on *Drosophila*. He obtained a triploid female ; that is to say, one with 3 chromosomes of each kind instead of 2 —produced probably from an egg-cell having the full somatic complement of chromosomes, and therefore twice the usual number, as the result of an irregularity during meiosis. When this triploid was bred from, some of the progeny were intermediate in their sex. The intermediates varied in character, some being more female in type and others more male, but they were easily distinguished from true males or females. Usually, sex-combs—a male character—were present on the tarsi of the fore-legs, the abdomen was intermediate between that of the two sexes, and the external genitalia were more like those of the

female ; the gonads were usually rudimentary ovaries, but sometimes one was an ovary and the other a testis. All of them were sterile. When they were examined cytologically it was found that a relation between the sex chromosomes and the appearance of the fly could be established.

It is not necessary to explain how the different types arose, but it may be stated that owing to irregular chromosome behaviour in the triploid its offspring contained various different chromosome complements, with perhaps one, two, or three, X chromosomes. The important factor in determining the sex of the various types was not, apparently, the absolute number of X chromosomes present, but the relation between the number of X and the number of each of the other chromosomes, the autosomes. In the ordinary diploid fly there are two sets of autosomes ; and these, combined with two X chromosomes give a female, with one X give a male. Or, calling a single set of autosomes A, then $2X : 2A$ gives a female and $1X : 2A$ gives a male. It might be expected that the triploid, $3X : 3A$, would be a normal female ; and this is the case, except for its large eyes and for small differences, not altogether unexpected, in its size and proportions. But individuals with chromosomes $2X : 3A$, that is to say a lower proportion of X chromosomes than the female and a higher proportion than the male, are intersexes. Two other types, $3X : 2A$, with more X chromosomes than the female, and $1X : 3A$ with fewer than the male, were also found ; and, on account of these formulæ, were called respectively super-females and supermales. They resembled, on the whole, females and males but were distinct types and were always sterile.

These observations are very important for two reasons. In the first place, they emphasize, that a factor, or chromosome, does not act independently but in co-operation with the other factors or chromosomes of the organism. When

we think of the pairs of factors *TT* and *tt* producing tall and dwarf in the sweet pea we must realize that they produce this effect only in conjunction with a particular body of genetical material. If the effect of the pairs of factors *TT* and *tt* could be studied when present with three sets of sweet pea chromosomes instead of two, it might be found to be somewhat different; and under still other circumstances the effect might be different again.

In the second place, they show that the appearance of an individual may be altered merely by altering the relative proportions of its factors or chromosomes, and without any change having occurred in the actual nature of any of the constituents. Many other examples of this have since been discovered.

The existence of intersexes shows that sex need not be a sharply-defined character, although at first sight it appears to be. In the case described the intersexes are the result of a genetical change; and in accordance with experience with other characters it may be suspected that under some circumstances intersexes might result from the influence of changes in the environment. The sex character, like so many others, might be subject to fluctuation.

This proves to be the case. Broadly speaking, it may be said that sex is determined genetically by the sex chromosome mechanism, but is much more sharply defined in some species than in others, apart from species in which the hermaphrodite is the normal form.

In some animals the environment has considerable influence on development. An extreme example is given by the marine worm *Bonellia*, in which Baltzer showed that a fertilized egg-cell develops into a female if it falls free, but into a male if it falls on to the proboscis of a female; so that in this case the genetical influences are evenly balanced and the sex is determined entirely by conditions.

Fluctuation is very marked in the frog. Various influences can affect the sex of an individual; and it is possible for an *XX* zygote that would be expected to become a female to develop into a male. As in some other species in which sex reversal is possible, the genetical constitution of an individual frog can be found out by breeding from it. Suppose that an *XX* zygote, which would develop into a female in normal conditions, comes under influences which make it develop into a male. This male, *XX* instead of *XY*, when mated with a normal female, also *XX*, gives offspring that are all *XX*; that is to say, its offspring will all be females if they develop under normal conditions. An *XX* male of this kind is often spoken of as a reversed female.

A less extreme example of fluctuation in sex is found in poultry, in which it is well known that an old hen past laying may begin to crow like a cock, and take on other characteristic features of the male.

In most animals fluctuation in the sex character is far less obvious than it is in the frog; and in many of them none has been detected, so that under known conditions intermediates do not occur and sex is determined completely by the chromosome constitution. In mammals, sex is fairly sharply defined but intermediates are known in a number of species, including man. A well-known case of partial sex reversal is found in cattle, which occasionally give birth to what is called a freemartin. This is of intermediate sex, and when found is always twin to a bull calf. Genetically it is a female; and the partial sex reversal is brought about by the hormones of its male twin. These have circulated through its blood—and therefore influenced its development—as the result of the partial anastomosis of the blood-vessels supplying the two embryos.

Goldschmidt, in Germany, has used the occurrence of

intersexes to investigate still further the mechanism of sex determination; and his results are important in the additional information they give about the way in which factors must be supposed to influence development. The intersexes he worked with arose when crosses were made between different species of the moth *Lymantria*, namely *L. dispar* and *L. japonica*. His conclusions will be described for the light they throw upon the development of intersexes, and for what can be learned about the way in which sex determiners produce their effects. There is no need in this connexion to discuss the genetical theory by which he sought to explain the way in which these various types bred.

The essential difference between the male and the female is in the gonads: the former has a testis, producing sperm, and the latter an ovary, producing egg-cells. There are also secondary differences, known as secondary sexual characters, which occur in different parts and organs of the body. Examples are the mammary, or milk-producing, glands in mammals, the plumage differences between cock and hen in birds, and so on.

The various differences between male and female are determined at different times during development. Development starts from two apparently identical fertilized egg-cells, and during the initial stages all embryos look alike. A difference in sex is first seen in the gonads. These begin their development alike but are early differentiated either as ovaries or as testes. Other organs develop differences later. In moths, the last difference to be determined is the pattern on the wings.

Goldschmidt found that these facts gave the key to the nature of the different types of intersex he found. These were of various grades, ranging all the way from male to female; either from male towards female, or from female

towards male. The various organs are affected differently in different grades. In the lowest grades of intersex—those that were only slightly changed from the true male or female —the only part affected was the wing pattern, the last character in the developing moth in which a difference between male and female develops, so that there would be an individual that was typically male or female except for a resemblance to the opposite sex in its wing pattern. With higher grades of intersexuality the wing pattern is always one of the affected parts, other organs are affected in a definite order, and the last to be affected is the gonads themselves, which may then show various stages of intermediacy in structure between a testis and an ovary. The gonads were the first to be differentiated during development; and it was found that all the organs are affected in the reverse order to that in which they are differentiated in the developing embryo.

To explain these results Goldschmidt compares the progress of differentiation to a chemical reaction which may vary in velocity. Since a male may develop traces of femaleness, and a female traces of maleness, he points out that any zygote must have the potentialities of both sexes; and which potentiality will prevail is determined by the sex factors it receives. During development there is a male reaction and a female reaction both going on independently in the same zygote. So long as the male reaction is the faster the zygote differentiates as a male, if the female reaction is faster it differentiates as a female; intersexes occur if one reaction is faster at the beginning of development but the other one prevails later.

In figure 13, it is supposed that the two reactions, male and female, have different rates at different stages of development. At first the male reaction is the faster and the zygote develops as a male. This continues until the point

S is reached. After this the female reaction is the faster; and the zygote no longer develops as a male but as a female. Any organ which has finished its differentiation before the time, represented by the point S, at which the male reaction becomes the slower will be entirely male. If, on the other hand, development is not finished by then it will start like a male organ and finish like a female; while if

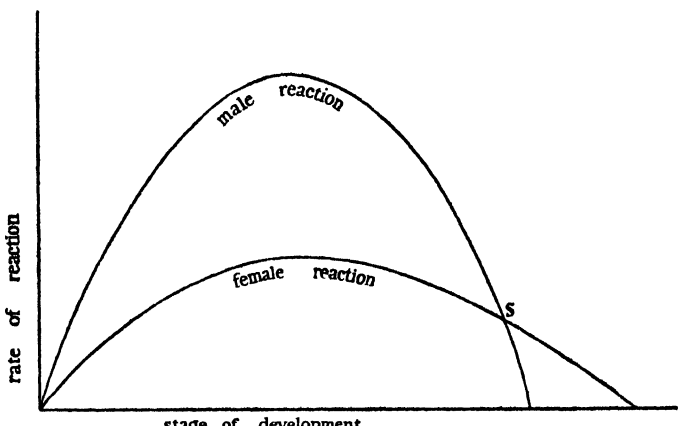


FIG. 13.—At first, the male reaction is the faster and the individual develops as a male. After passing the stage in development represented by the point S the female reaction is the faster and the individual completes its development as a female.

differentiation does not occur until very late in development, later than that represented by the point S in the diagram, it will be entirely female. Wing pattern in *Lymantria* is a character that is differentiated so late that in low-grade intersexes there may be a female wing pattern in an individual that is otherwise male, or a male wing pattern in an individual otherwise female. Evidently, the earlier the point at which the two curves cross over the higher will be the grade of the intersexuality.

Goldschmidt's theory gives a simple way of visualizing the determination of sex. It interprets satisfactorily not only the existence of intersexes, but also the fact that the grade of intersexuality may differ in different organs of the same individual. No doubt the same conception can be usefully applied to the development of many other characters that fluctuate, and examples are in fact frequently met with during genetical experiments.

The conception has been used successfully in interpreting the development of eye colour in *Gammarus chevreuxi* (a "shrimp" of brackish waters). The eyes of this species may be black or red, and development appears to depend upon the action of genetical factors that accelerate or retard the deposition of black pigment. Incidentally, it is interesting to notice that the speed of the process may be affected in exactly the same way by raising or lowering the temperature.

No doubt other cases, to which the same view does not apply, will be found; even on Goldschmidt's supposition it is only reasonable to suppose that development depends not only upon the rate at which chemical reactions occur but also upon the nature of the reaction.

To recapitulate, we saw in the last chapter that cytological study had made clear how the chromosomes transmit Mendelian factors from one cell to its descendants, and therefore from one generation to the next. At somatic divisions the chromosomes divide equationally, ensuring that the cells making up the body of an organism are all genetically identical. At the reduction divisions they pair and segregate, bringing about segregation of the Mendelian factors, and reducing their own numbers so that the gametes have half the number found in the body-cells. That chromosomes were important in heredity was suggested before the rediscovery of Mendelism, partly because of

their evident importance in cell division, partly because of experiments designed especially to show that the nucleus, and not the cytoplasm, was responsible.

Still stronger evidence is given by the many cases in which an unusual factor segregation is accompanied, and presumably determined, by an exactly similar irregularity in chromosome segregation. This is best shown in cases yet to be described ; but sex, and sex-linked inheritance, are simple examples.

It was originally suggested that sex was determined by a whole chromosome, but later work showed that it is more fitly described as dependent upon the relation between the sex chromosomes and the other chromosomes of the complement. Like so many other characters sex is subject to fluctuation in some species, so that sex intermediates may exist. These intermediates are sometimes caused genetically, and analysis of them has suggested that the effect of the sex chromosomes in bringing about differentiation of sex characters may be likened to that brought about by altering the rate of a chemical reaction.

It may be pointed out in conclusion, however, that although the genetical analysis of sex has gone far it is deficient in one important respect. The remarkable feature of the development of sex is that a zygote should often, apparently, be able to develop either into a perfect female or into a perfect male. In some species, as in *Bonellia*, the environment decides which of the two will be formed ; and in others genetical influences are paramount. Something has been learned about the genetical influences that decide the issue. Nothing is known about the origin of the zygote's potentiality for developing into either of two highly specialized, and very different, organisms : the male, and the female.

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CHAPTER VII

LINKAGE

AT the very outset, the theory that the chromosomes bring about the segregation of Mendelian factors had one difficulty to face. Factors commonly segregate independently of one another. When a form carrying two factors such as *A* and *B* is crossed with the double recessive there is no tendency for *A* and *B* to remain together; the F_1 produces recombination gametes as often as it produces the original parental gametes. This would occur on the chromosome theory if *A* and *B* are carried by chromosomes belonging to different pairs; for the members of different pairs segregate independently. It would not be expected to occur, however, in the case of factors carried by the same chromosome. In this case, two factors must always remain together unless the chromosome carrying them breaks up in some way.

But if factors in the same chromosome always remain together, then the number of factors that segregate independently cannot be greater than the number of chromosome pairs. In the early days of Mendelism, however, it seemed very unlikely that this would prove to be true. Mendel himself had discovered 7 independent pairs of factors in the pea; later research showed that there were 4 more pairs, also apparently independent, making 11 in all. Yet there are only 7 pairs of chromosomes in this species.

The clue to this problem is the phenomenon known as *linkage*, first discovered in the sweet pea by Bateson and Punnett, and afterwards incorporated in the chromosome theory by the American biologist Morgan.

Bateson and Punnett were investigating the inheritance of pollen shape in the sweet pea, and crossed together a race with long pollen and one with round pollen. (There were two distinct possibilities with this character. Supposing, as proved to be the case, that only one factor, *L*, was involved ; and that long *LL*, was dominant to round, *ll* ; then the heterozygote, *Ll*, distributes *L* to one-half the pollen and *l* to the other half, and it seemed possible that the half carrying *L* might be long and that carrying *l* round. On the other hand there was also the possibility that pollen shape would prove to be a maternal character, depending upon the genetical constitution of the mother plant, and not on that of the individual pollen grains ; this second alternative proved correct. The pollen grains of one plant were always alike. The heterozygotes, *Ll*, had only long pollen ; and the F_2 generation consisted therefore of plants, *LL* or *Ll*, with long pollen only and plants, *ll*, with round pollen only, the two types occurring in the ratio of 3 : 1.)

In the case studied, the two parents differed by other factors as well as those for pollen shape, and the transmission of these was followed at the same time. One of the characters involved was flower colour, and it was noticed that the segregation of purple flower colour and long pollen was not independent. Considered apart from one another these characters were behaving quite normally. The F_2 contained 3 purple : 1 red, and 3 long : 1 round. But far too many of the purple-flowered plants were found to have long pollen, and too few of the red-flowered. (There should have been 3 long : 1 round among both classes, but

among the first there were about 12 long : 1 round, and among the second about 1 long : 3·2 round. Altogether the four classes were in the ratio 11·4 purple long : 0·8 purple round : 0·8 red long : 3·0 red round, instead of 9 : 3 : 3 : 1, which would have been the ratio if long and purple had been independent, as all the characters studied up to that time had been. The classes that were in such great excess, it will be noticed, were the two parental classes, purple long and red round, which entered the cross together ; the two recombination classes, purple round and red long, being in defect.

purple long, *BBLL* × red round, *bb ll*

↓

F_1 *Bb Ll*

↓

F_2 found

11·4 purple long : 0·8 purple round : 0·8 red long : 3·0 red round

F_2 expected if the factors were independent

9 purple long : 3 purple round : 3 red long : 1 red round

It will be noticed that

total purple : total red = (11·4 + 0·8) : (0·8 + 3·0) = 3 : 1 approx.

„ long : „ round = (11·4 + 0·8) : (0·8 + 3·0) = 3 : 1 „

In seeking an explanation of these results, it may be assumed from the 3 : 1 ratios found in F_2 that both *L* and *l*, and *B* and *b*, are segregating quite as usual, and give F_1 gametes in the ratios of 1 *L* : 1 *l* and 1 *B* : 1 *b*. Considered independently the two pairs of factors are behaving normally ; it is only in their relations with each other that there is any peculiarity.

There are several possible ways in which a result of this kind might have been explained ; but that suggested by Bateson and Punnett, which has since proved correct, was that the parental types, *B* and *L* or *b* and *l*, were associated in some way, so that instead of the gametes being produced

in the ratio 1 *BL* : 1 *Bl* : 1 *bL* : 1 *bl* they were produced in a ratio of about 8 *BL* : 1 *Bl* : 1 *bL* : 8 *bl*, the parental gametes being in excess.

F_1 *Bb Ll*

F_1 gametes (if *B* and *L* independent) 1 *BL* : 1 *Bl* : 1 *bL* : 1 *bl*
F_1 gametes actually produced 8 *BL* : 1 *Bl* : 1 *bL* : 8 *bl*
F_2 from random mating would be $(8\,BL + 1\,Bl + 1\,bL + 8\,bl)^2$
and it can be calculated that this would give, approximately, the actual ratio found—
11·4 purple long : 0·8 purple round : 0·8 red long : 3·0 red round.

In other words, *B* and *L* do not segregate independently. There is a tendency for the factors that enter the cross together to remain together, and for fewer recombination gametes to be formed. This phenomenon is known as *linkage*. It can be measured by the proportion of recombination gametes, the usual measure now in use being the quantity $\frac{\text{number of recombination gametes}}{\text{total number of gametes}} \times 100$, known as the *percentage of crossing-over*. In the example just given, the percentage of crossing-over is approximately

$$\frac{1+1}{8+1+1+8} \times 100, \text{ or } \tfrac{2}{18} \times 100 = 11{\cdot}1 \text{ per cent}$$

Linkage is best measured from a back-cross; and not from the usual F_2, obtained by selfing. If the F_1 be crossed to the double recessive, the zygotes obtained can be classified at once and appear in the proportion in which the F_1 gametes are formed. The heterozygote *BbLl* produces gametes *BL*, *Bl*, *bL*, and *bl*; and these, uniting with *bl* from the double recessive, give purple long, purple round, red long, and red round, plants in the proportions in which the gametes themselves were produced. The F_1 could be used both as male and as female parent in making the cross; and this would show immediately whether there were any difference in crossing-over value in male and female, a

fact which could only be detected with difficulty in an F_2, and might easily be overlooked. In other ways also an F_2 gives the cross-over value less directly than a back cross, and the calculation is less exact and more difficult to make, so that back crosses are used in linkage work whenever possible. However, there are many plants in which the labour required for this method is so considerable that an F_2 has to be employed instead.

To explain linkage on the chromosome theory it was supposed by Morgan that linked factors are carried by the same chromosome, and that when two homologous chromosomes pair at meiosis they may form recombination gametes by some sort of interchange of material. A chromosome from one parent, carrying *AB*, pairs with a homologous chromosome from the other parent, carrying *ab* ; and the recombination gametes *Ab* and *aB* would sometimes be formed by an interchange or *crossing-over* between these two chromosomes, as in figure 14 (p. 107).

This theory was first worked out in the fruit-fly, *Drosophila melanogaster*, which, in the hands of the American workers, has proved itself admirably suited to genetical experiment. In general appearance this fly is not very different from the common house-fly, but is only about one-fifth the length. It thrives on fermenting bananas, and a large family can easily be reared in a glass bottle of half- or one-pint capacity. At a temperature of about 24° to 25° C. a new generation, from adult to adult, can be obtained in about 10 days ; and under favourable conditions 25 generations can be reared in a year. Moreover the produce of a single mating will commonly be some two or three hundred flies. This rapidity in breeding, so much above that of most organisms, is naturally a great advantage in genetical work. Not only can experiments be carried out for many generations if needed, but a theory or conclusion can be tested in a reason-

ably short time instead of having to wait for several years, as with most species, for the results of each experiment. When the flies are to be classified, they are first anæsthetized by keeping them in ether for about ten seconds, then shaken out on to a plate and examined under a binocular microscope.

D. melanogaster was the first species in which the linkage between factors was studied on a large scale. It was found, and the same has since been shown for other species, that the factors fall into a number of different linkage groups. Thus if *A*, *B*, *C*, *D* form one group and *M*, *N*, *O*, *P*, *Q*, *R* another, then after a cross the factors in the same group—*A* and *B*, *B* and *D*, *N* and *Q*, and so on—will be linked to one another and will segregate independently from the factors in any other group. If it has been shown that in one cross *A* is linked to *B* and in another that *B* is linked to *C*, then it can be predicted that *A* will be linked to *C*.

This result might have been expected on any theory, so long as linkage is an orderly process ; but what is remarkable is that in *Drosophila*, in which the inheritance of several hundred factors has been studied, the number of linkage groups is the same as the haploid chromosome number, four. No other species has been so well analysed as *D. melanogaster*, though there are several in which a fairly large number of factors are known and a similar result has been obtained. In the sweet pea, *Lathyrus odoratus*, in which the haploid chromosome number is seven, Punnett has shown that the nineteen known factors fall into seven linkage groups ; in maize, *Zea mays*, with $n = 10$, about ninety factors falling into nine groups are so far known, the tenth group not yet having been identified. Furthermore, there are a number of other species in which the evidence for a connexion between the haploid chromosome number and the number of linkage groups, though much less definite, is at least confirmatory, since the number of factors that

segregate independently is never greater than the haploid chromosome number.

In many ways the cytological theory of linkage and crossing-over has been made very precise; but the exact way in which two chromosomes, *AB* and *ab*, cross over to give the recombinations *Ab* and *aB* is not known though various suggestions have been made. It may be said with consider-

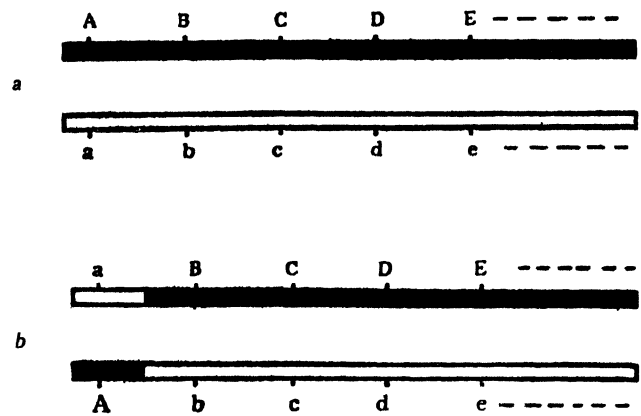

FIG. 14.

a, two homologous chromosomes before crossing over; *b*, after crossing over.

able confidence, however, that crossing-over almost certainly occurs during the early prophase of meiosis, at a stage when the chromosomes are in the form of long thin threads and those that are homologous have paired with one another. Morgan supposed that the factors are arranged in linear order along these threads, like beads on a string, and has shown that this assumption makes it possible to explain the facts of linkage.

Suppose that one chromosome carries the factors *A*, *B*, *C*, *D* . . . in that order, and its homologue the corresponding allelomorphs *a*, *b*, *c*, *d* . . (fig. 14*a*).

At the prophase of meiosis these 2 chromosomes pair with one another.

If there were no interchange, or crossing-over, between them, then the factors would segregate in completely linked groups. But a cross-over between *A* and *B* would give the two recombination classes *aBCD* and *Abcd* (fig. 14*b*).

If crossing-over is equally likely to occur anywhere along the chromosome, then the frequency with which it occurs between any two factors will depend upon how far they are apart. A cross-over between *A* and *E*, in figure 14, would be four times as likely as one between *A* and *B*; that is to say, the gametes *Ae* and *aE* would be formed four times as often as *Ab* and *aB*. If *A* and *B* are close together, we may suppose that out of every 100 gametes 99 would be the original combination *AB*, and 1 the cross-over combination *Ab*, that is crossing-over is 1 per cent. Between *A* and *E* we should then expect 4 per cent. of crossing-over—out of every 100 gametes there would be about 4 *Ae* and 96 *AE*. Furthermore, the cross-over value for *AC* would be the sum of the two cross-over values *AB* and *BC*. //

These simple theoretical expectations are, to a fairly close approximation, realized in practice. It therefore follows that, if the linkage values of a series of factors are found experimentally, the factors can be arranged in linear order in the way shown above. In this way a diagram, usually spoken of as the chromosome "map," can be constructed showing the position that must be occupied by each factor to agree with the linkage data.

The amount of crossing-over between two factors is affected by temperature, and, in *Drosophila* at any rate, by the age of the individual when meiosis occurs, and by other factors both environmental and genetical. When allowance is made for these effects it is found, not unexpectedly, that there are complications to the simple expectation

described. The first of these is a consequence of the fact that more than one cross-over can, and normally does, take place in the same chromosome; (the general effect being somewhat to reduce the amount of crossing-over between two factors a long distance apart.) A further complication, known as interference, is that when one cross-over has already occurred another is not likely to take place in the immediate neighbourhood of the first.

Because linkage is not a haphazard process, but a regular one, it has been possible to interpret it in terms of chromosomes when certain assumptions are made; and there were in the past two lines of evidence favouring the interpretation. The first was that in *Drosophila* the number of linkage groups equals the reduced chromosome number; the same will no doubt be proved for many other organisms. The second was that linkage data agree with the assumption of a linear arrangement for the factors, so harmonizing with the supposed differentiation of chromosomes along their length. Taken alone, these facts would do no more than make the chromosome theory of linkage a plausible hypothesis; but an experiment carried out by Curt Stern, at the Kaiser Wilhelm Institut für Biologie in Berlin, comes as near complete proof as it would be possible to get.

If two homologous chromosomes are represented by *AB* and *ab*, the fact of crossing-over with the production of *Ab* and *aB* could be demonstrated if the 2 chromosomes were recognizably different at both ends. It was by this method that Stern was able to show that a genetical crossing-over is accompanied by a recognizable cytological crossing-over.

A hypothetical case of this kind, in which crossing-over has given two chromosomes recognizably different from the original two, is shown diagrammatically in figure 15 (p. 110). The material for Stern's actual experiment was obtained from

two races of *Drosophila* that differed in their *X* chromosomes. From these he built up a strain having, in the females, one *X* chromosome fragmented, that is to say, in two separate portions, and the other with part of a *Y* chromosome attached to one end (fig. 16*a*). The fragmented *X* carried the factors *B* and *cr*, each of which was located near the end of one of the fragments ; and the same fragment differed visibly at both ends from the whole chromosome, having attached *Y*, with which it paired at meiosis. Then, if crossing-over took place between *cr* and *B* the germ-cells

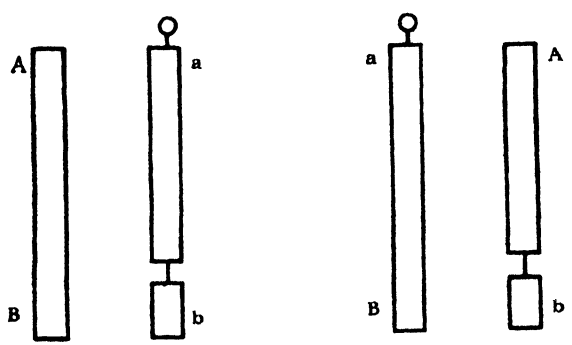

FIG. 15.

The cross-over chromosomes *aB* and *Ab* differ visibly from *AB* and *ab*.

of such a female would contain, besides the two original *X* types (figs. 16*d* and *e*), 2 new chromosomes formed by crossing-over, one carrying the factor *B* (fig. 16*c*) and the other one the factor *cr* (fig. 16*b*). *B* is dominant and *cr* is recessive ; so that, if the female producing these germ-cells is crossed to a male with a normal *X* chromosome carrying the factors *b* and *cr*, the four different types of germ-cell will show their characters (whether *cr* or normal, and whether *B* or normal) in the females they produce(fig. 16*f*), and these females can be classified both by their appearance and by the shape of their chromosomes. Exceptions would occur only if crossing-

over had taken place not between *B* and *cr* but between *cr* and the end of the fragment, giving two types that would be genetically the same as the two parents but cytologically

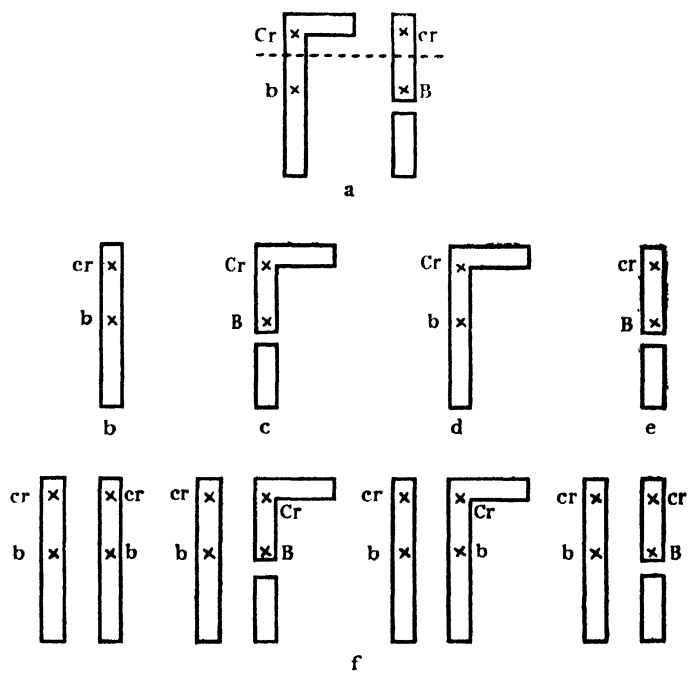

FIG. 16.—*a*. Two visibly different *X* chromosomes, one being fragmented, the other having attached *Y*. Segregation, with or without crossing-over at the level shown by the dotted line, gives gametes with the four different *X* chromosomes shown in *b*, *c*, *d*, and *e*; and these, with a normal *X* carrying *cr* and *b*, give the four different combinations shown in *f*, which can be recognized not only by their chromosomes but by the factors they carry.

different. In the relatively small number of flies examined, 59 in one experiment, none of these exceptions was found: agreement between genetical and cytological examination

was complete. This experiment, therefore, not only shows that crossing-over between two homologous chromosomes really does occur ; it also shows that cytological crossing-over is accompanied by genetical crossing-over ; and it may safely be assumed that the factors are arranged along the chromosomes in linear order as Morgan supposed.

In certain cases linkage is able to assist genetical analysis. An example is given by modifying factors, mentioned on p. 64. These are difficult to analyse and identify in the usual way because their effect is small compared with that of the environment. The existence, and approximate position on the chromosome, of a modifying factor may be established, however, because all factors close to it will also have on the average an effect on the character it modifies, and this effect will get less with factors that are further away. In *Drosophila* especially this method of analysis has been adopted with considerable success.

Further inquiry has naturally been given to finding out exactly how crossing-over takes place. This has not yet been discovered, but study of the prophases of meiosis has shown how something more can be learned about the linear differentiation of the chromosomes.

The meiotic prophase is difficult to study. It is a period in which the chromosomes are like long, fine threads. These threads are, in any case, difficult to observe accurately because their thickness is little above the limit of microscopic visibility ; an added difficulty is that the nucleus is very sensitive to fixatives during this period, and the chromosome threads are very apt to collapse together on to the side of the nucleus. However, careful study has made the general outlines of chromosome behaviour during these stages familiar ; and in recent years better methods of fixation, and a sounder knowledge of genetical principles, has made a further advance possible, so that most of the

basic principles of chromosome behaviour during the meiotic prophase are now well understood.

In the earliest prophase of meiosis the nucleus contains a number of long and very fine threads which, from their later behaviour, must be the chromosomes. These threads come together in pairs, the association beginning at random at any point along the pairing chromosomes; and it is

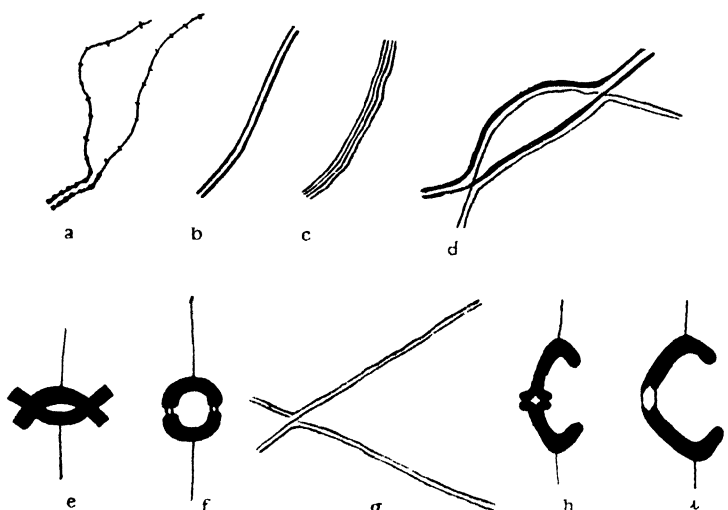

FIG. 17.

d. One pair of threads drawn more thickly for clarity, chromosomes in *e* and *f* have developed from *d*; in *h* and *i*, from *g*.

believed, as the work of Newton and Darlington, at the John Innes Horticultural Institution at Merton, first suggested, that this pairing occurs particle by particle between particles that are homologous and only when they are homologous. As pairing occurs the chromosomes thicken (fig. 17*a*), and when it has been completed (fig. 17*b*) each chromosome splits longitudinally into two *chromatids* (fig. 17*c*). The four chromatids from one pair do not all

remain associated; pairing persists only between any two of them, so that they fall apart in pairs, and if different threads remain together at different points an exchange of partner occurs (fig. 17*d*). The points at which threads change partners are known as *chiasmata*, a term introduced by Janssens, who was the first to recognize these points as exchanges of partner between paired chromatids. From now until metaphase the paired threads, or chromosomes, shorten and thicken continuously and are held together, as Darlington has shown, by these chiasmata. In most organisms, as the chromosomes contract the chiasmata move away from the attachment constriction—that is to say, the point at which the spindle fibre will be attached—and towards the ends. At metaphase the paired chromosomes may therefore have various shapes according to the number of chiasmata originally formed and whether they have all reached the ends or not. Some differently shaped metaphase bivalents are shown in the figures (figs. 17*e*, *f*, *h*, *i*).

What connexion there may be between chiasmata and genetical crossing-over has not yet been decided. It was early suggested that genetical crossing-over, which of course involves an exchange of material between two chromosomes, might be explained if a break occurred at a chiasma, but on the view of meiosis described above it may be assumed that chiasmata do not break; and some workers believe, correspondingly, that crossing-over has no connexion with chiasmata. Darlington considers, on the other hand, that crossing-over occurs in the earlier prophase stages and that chiasmata are formed in consequence, at the points where crossing-over had occurred.

Whether or not chiasmata and crossing-over are in any way connected, the results described have shown that definite conclusions can be reached about the connexion between linkage and chromosomes. In the first place, we

have seen that linkage between factors can be expressed by arranging them in linear order in separate groups; this, according to the chromosome theory, shows their linear position on the chromosomes. Secondly, cytological observations suggest that chromosomes pair particle by particle so long as corresponding particles are homologous.

These two theories have an interesting consequence which has been realized experimentally in recent years. Suppose that factors *ABCDEF* form a single linkage group arranged in this order on one chromosome, and *KLMNOP* a second group arranged in that order on another chromosome. It

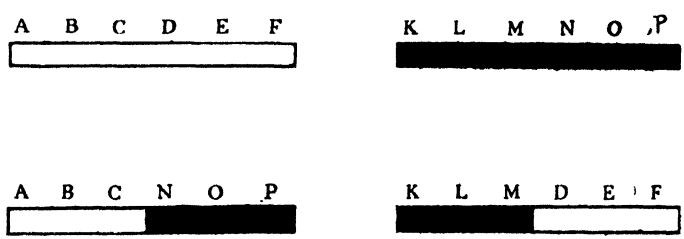

FIG. 18.

has been found in some species that races exist that differ in the arrangement of the factors in the chromosomes, so that *ABCNOP* forms one group and *KLMDEF* another, instead of *ABCDEF* and *KLMNOP*; exactly as if the two chromosomes concerned had interchanged half their material (fig. 18), *segmental interchange* as it is called.

Two such races are exactly alike in their outward appearance; for both have the same factors, and it is only in the arrangement of these factors that there is any difference. The difference between them is revealed if they are used for studying linkage relations; and is also shown—and it is this that interests us at present—when the two races are crossed together. The F_1 from the cross between them

contains 4 chromosomes—*ABCDEF*, *ABCNOP*, *KLMNOP*, *KLMDEF*—of which no two are the same, each having one-half homologous with one-half of a second chromosome and the other half homologous with one-half of a third chromosome ; *ABCDEF*, for example, is homologous half with *ABCNOP* and half with *KLMDEF*. These 4 chromosomes can pair together as in figure 19*a* at prophase. Chiasmata can arise between the parts that are paired, and if these chiasmata move towards the ends of the chromosomes

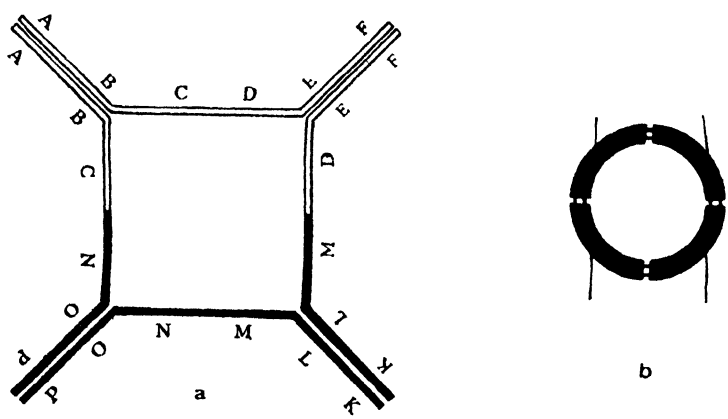

FIG. 19.

the result at metaphase will be 4 chromosomes associated together in a closed ring (fig. 19*b*).

This result has been found to occur in several genera of plants. In *Pisum* it has been worked out both genetically and cytologically by Hammarlund and Håkansson in Sweden, and by Pellew and Richardson in England. The factors *A*, for violet as opposed to white flowers, and *Gp*, for green as opposed to yellow seeds, were found to be transmitted independently of one another in some strains

of pea ; and in these strains must be carried by different chromosomes. In other strains they were found to be closely linked, the cross-over value being about 1·7 per cent., and must be carried by the same chromosome. Crosses between the two kinds of strain gave, as expected, plants having a ring of 4 chromosomes.

A further point of interest is that the latter plants are partially sterile, about half the pollen and ovules being ineffective. This arises because the chromosomes in the ring, supposing that two of them go to one pole and two to the other, which is usually the case, can segregate in two ways. If opposite chromosomes go to the same pole, that is to say *ABCDEF* and *KLMNOP* to one pole and *ABCNOP* and *KLMDEF* to the other, then the gametes formed are fertile. But if adjacent chromosomes go to the same pole, for example *ABCDEF* and *ABCNOP* or *KLMNOP* and *KLMDEF*, then the gametes produced will be sterile because part of a chromosome is completely lacking ; in the example given *KLM* is lacking from the first gamete and *ABC* from the second.

The example described is only one of the ways in which genetical material may be differently organized in different races. In *Drosophila*, Bridges has worked out cases in which various departures from the normal arrangement can be found. Examples are *duplication*, in which part of a chromosome is duplicated, *translocation*, in which part is found associated with a different chromosome, and *deficiency*, in which part is completely lacking. Several types of rearrangement, and the kinds of association between chromosomes at meiosis to which they might give rise, have been worked out in the thorn-apple or Jimson weed, *Datura stramonium*, by Belling in the U.S.A.

These types of variation are important because they are a newly discovered kind which has definite effects not found

in the cases of ordinary factor mutation. This aspect of the matter will be discussed later in connexion with the differentiation between species ; at present it has been described because it shows that, when factors are arranged into different groups in different races of the same species, there appears to be a corresponding difference in the organization of the chromosomes, and this has a definite, expected, effect on the pairing of the chromosomes.

There can now be no doubt that the segregation of Mendelian factors is brought about by the chromosomes ; and will be subject to any irregularity that chromosome segregation may be subject to. In sex-linked inheritance, a departure from the usual method of factor transmission follows a specialized method of chromosome differentiation and transmission. The segregation of 1 *A* : 1 *a* found by Mendel is merely a simple, special case, occurring when chromosome behaviour is regular.

The 3 : 1 ratio of simple Mendelian inheritance is also disturbed by other phenomena. Supposing that *A* and *a* are segregated to the germ-cells in the ratio 1 *A* : 1 *a*, a further assumption needed to give the ratio of 1 *AA* : 2 *Aa* : 1 *aa* is that both the *A* and the *a* germ-cells survive and mate together at random. It will be shown later that in certain cases, notably crosses between species that give partially sterile hybrids, these assumptions no longer hold. Moreover, as the case of the lethal factor shows, even if the three classes of zygote—*AA*, *Aa*, and *aa*—are all produced it does not follow that all are equally viable.

The only essential feature of Mendelian inheritance is that the units concerned, the factors *A* and *a*, remain constant and are transmitted unchanged from one generation to the next.

It is this assumption which appears to have general validity ; and it is this assumption that makes exact analysis

possible. Otherwise every possible irregularity may be found. But these are of secondary importance. They merely disturb the ratio produced, and do not prevent analysis. Indeed, it is only in special circumstances—that of the fertile hybrid between closely related forms—that the simple conditions found by Mendel will occur.

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CHAPTER VIII

CHROMOSOME VARIATION

SO far variation has been ascribed, when genetically significant and not merely caused by the environment, almost entirely to the action of Mendelian factors. The dwarf pea differs from the tall in possessing a factor *t* instead of *T*. Brief mention was made, however, of variation in the organization of factors into chromosomes.

An entirely different kind of variation is variation in the number of chromosomes. This is very widespread and since about 1920 has attracted much attention, especially in plants.

Usually the number of chromosomes in an individual, and in different individuals belonging to the same species, is constant; as might be expected from the way in which they divide, and from the general body of evidence that they are permanent structures. The same is true sometimes of larger groups; but, on the other hand, it often happens that different species have different numbers of chromosomes. The various kinds of *Chrysanthemum*, for example, may have 18, 36, 54, 72, or 90 chromosomes.

Variation in chromosome number is important because it gives rise to phenomena that do not come from factorial changes alone. When two forms, *ABC* . . . and *abc* . . ., differ even by a large number of Mendelian factors, they may be very unlike in appearance but it is possible by crossing them to obtain a series of transition forms—*Abc*

. . ., *aBc* . . ., and so on—which have the character differences in any combination. When forms differ in chromosome number, on the other hand, not only may they be distinctly different in appearance but also they may not give transition forms when crossed.

When the chromosome numbers found are all simple multiples of some basic number, as in *Chrysanthemum*, they are said to form a *polyploid* series. In this genus the basic number is 9, the number found in the gametes of those forms with the lowest number. This is said to be the *haploid* number, and is denoted by x. It is the number found in plants having the *diploid* number, 18 in *Chrysanthemum*, in their body cells. The other multiples of the basic number, such as 27, 36, 45, and 54 in *Chrysanthemum*, are spoken of as *triploid*, *tetraploid*, *pentaploid*, *hexaploid*, and so on; and may be denoted $3x$, $4x$, $5x$, and $6x$.

Before polyploid series were discovered and studied, the number of chromosomes found in the body-cells of any species was called the diploid number, and the number in the gametes the haploid number; but it is better to use only the terms somatic number and gametic number for these, and to define the terms haploid and diploid as above, since confusion may otherwise arise. The somatic number may be denoted by $2n$, and the gametic number by n. The somatic number, $2n$, of a tetraploid *Chrysanthemum* is then 36, $= 4x$; and the gametic number of the hexaploid is 27, $n = 3x$, the gametes being triploid.

Numbers that do not go up in any regular progression, such as 6, 9, 10, 11, 12, 13, some of the gametic numbers found in different species of *Viola*, the genus to which the Pansy belongs, are said to form an irregular series. Examples are common both in animals and in plants. Thus, the different species of *Drosophila* may have the numbers 3, 4, 5, or 6.

Polyploid series are common in the plant kingdom, and have been studied in some detail during the last ten years, so that both their origin and special features are now fairly well understood. The simplest case, which will be dealt with first, is exemplified in the commonly cultivated Chinese primrose, *Primula sinensis*. The ordinary diploid form of this species has 12 chromosomes in its gametes and 24—two similar sets, of 12 each—in its somatic cells ; but a tetraploid form, with 48 chromosomes in its somatic cells instead of 24, is also known. This tetraploid is almost exactly like the diploid in appearance, and all the evidence suggests that this is because the only genetical difference between the two is that one possesses the set of 12 *P. sinensis* chromosomes four times and the other possesses it only twice. Cases of this kind are known as autotetraploids, or *autopolyploids*. In these, diploid and tetraploid differ only in the quantity of genetical material they contain ; the kind is the same in the two cases. Under these circumstances there is no reason for the two forms to differ at all in appearance, except that the tetraploid may have larger nuclei and larger cells, and may therefore be a rather larger plant. This is, in fact, the general experience with autotetraploids, though there may sometimes be small physiological differences between them and the diploids.

Two other circumstances, besides the similar appearance, favour this interpretation of the nature of autotetraploids. The first of these is that when the reduction divisions occur the chromosomes do not associate only in pairs as they do in the diploid. Pairs are frequently found ; but the chromosomes also come together in threes or fours, to give trivalents and quadrivalents. This is exactly what would be expected if there are four chromosomes of each kind instead of two, and if chromosomes pair because of their similarity. The exact association found varies somewhat

from cell to cell, merely through chance. In *P. sinensis*, complete association into 12 quadrivalents is rare, that most frequently found being 10 quadrivalents plus 4 bivalents. It was the observations made by Newton and Darlington on the pairing of chromosomes in polyploid forms that led to a proper understanding of many features of meiosis which were previously obscure.

The second line of evidence that has shown the proper nature of autotetraploids is the way in which they originate. It is true that direct evidence is not complete, but there can be little doubt as to the principle. In *Datura stramonium*, the thorn-apple, the ordinary diploid plant sometimes gives an isolated branch which is tetraploid, producing tetraploid seeds, from which tetraploid plants, in turn, arise. A branch of this kind would have come from a single bud; and the origin of this bud was probably a single tetraploid cell. Occasionally, indeed, a cell with double the number of chromosomes usual for the species is actually observed in actively growing tissue, such as a root tip. The occurrence being rare, it is not likely that the origin of such a cell would be observed; but it is plausible to suppose that it came from some irregularity in mitosis, such as a suspended division, which gives a nucleus identical with that of the normal diploid, except that every chromosome is present four times instead of twice.

Besides *Primula* and *Datura*, autotetraploids have been found in many other plants. In *Campanula persicifolia* the form grown under the name "Telham Beauty" is a tetraploid. It is a robust plant with rather larger flowers than the ordinary form. The first tetraploid to excite attention was found in *Oenothera lamarckiana*, the Evening Primrose. Towards the close of the last century De Vries observed that this species, when bred on a large scale, gave a number of new forms, of which one, called *gigas*, was distinguished from the

normal only by its larger size. This new type was shown later by Gates, at London University, and by Lutz in U.S.A., to have 28 chromosomes instead of the usual 14.

In some species autotetraploids can be induced artificially. If the stem of an actively growing young plant is cut across, and all the buds below the cut are removed, the cut surface forms a wound callus from which new buds are produced. When these buds grow out into new shoots it is found that some of the shoots are tetraploid, having come from a tetraploid cell produced in some way as a result of the wounding.

In *Datura stramonium*, besides the diploid and tetraploid forms, haploids and triploids are known. The haploids, $1x$ with 12 chromosomes, are produced occasionally by the normal diploid as the result of parthenogenesis, that is to say, the development of an egg-cell without fertilization. They have also come sometimes in other plants, in the same way, usually after an unsuccessful pollination by a distantly related species. The triploid, $3x$ with 36 chromosomes in *Datura*, is easily produced by crossing the tetraploid and the diploid, a $2x$ egg-cell from the former uniting with a $1x$ male gamete from the latter. For reasons that will be explained later, both the haploid and the triploid forms are largely sterile and therefore have characteristically shaped fruits, since in *Datura* the development of the fruit is partly dependent upon seed production. The tetraploid, also, is not quite so fertile as the diploid, with the result that all four forms can be recognized by the shape of their fruits. They also differ in size, but except for these two features are identical in appearance.

It is difficult to say how common autopolyploids are in nature, since even when they occur they are likely to be overlooked ; but they are probably of limited importance. The polyploid series found so commonly are of a different

kind, in which the forms with different numbers, instead of being alike are usually markedly different, often forming separate and distinct species. Well-known examples are wheat and oats, both of which have species with 14, 28, or 42 chromosomes. Wheat, for example, is a very variable plant, widely grown over the Old World from China to Western Europe, and usually divided into a dozen or so species. These species fall naturally into three groups, with 14, 28, and 42 chromosomes respectively.

1st Group, 14 Chromosomes.	2nd Group, 28 Chromosomes	3rd Group, 42 Chromosomes.
T. aegilopoides (wild form)	*T. dicoccoides* (wild form)	*T. vulgare* (bread wheat)
T. monococcum	*T. dicoccum*	*T. compactum*
	T. durum (macaroni wheat)	*T. sphaerococcum*
	T. turgidum	*T. spelta*
	T. persicum	
	T. polonicum	

Within the groups it is sometimes hard to define the limits of the different species; but the three groups themselves are easily set apart from one another, and if they had originated like the autopolyploids already described this would not have been the case; instead, the 28 chromosome species would have looked exactly like those with 14, and so on. It could be supposed that the three groups have become different since they first originated, but in many ways this is not very easy to understand, and it becomes still more unlikely when it is realized that in nature it is usual for the members of a polyploid series to be clearly separated from one another.

A much more satisfactory explanation of the origin of polyploid species is that put forward by the Danish geneticist Winge in 1917. He supposed that the basis of the process was a chance hybridization between two related

species, *A* and *B*, with either the same or different numbers of chromosomes. If these have chromosome numbers of $2a$ and $2b$ respectively, then the hybrid between them will have $(a + b)$ chromosomes, a from one parent and b from the other. Two forms with 8 and 14 chromosomes, for example, would give a hybrid with $4 + 7 = 11$. This hybrid, as we shall see later, not having two homologous sets of chromosomes, would probably be sterile. But Winge supposed that a doubling in chromosome number might take place, to give a fertile form with twice the chromosome number, $2a + 2b$ or 22, of the original hybrid. This new form would contain the complete chromosome complements of the two original parents, and would be fertile because it had two homologous sets of chromosomes, $a + b$ and $a + b$.

Species *A* with $2a$ chromosomes × species *B* with $2b$ chromosomes
↓
sterile hybrid with $(a + b)$ chromosomes
↓
new fertile form with $(2a + 2b)$ chromosomes

In this way two diploids crossed together would give a diploid hybrid, probably sterile; and from this a new, fully fertile tetraploid form could arise by chromosome doubling. In the same way a tetraploid crossed by a diploid would give a triploid F_1 from which a hexaploid could arise. Polyploids of this kind are called *allopolyploids*.

Winge's theory was neglected for some years, partly because he had given no reason why doubling of chromosome number should occur in the original hybrid, and partly because there was no direct evidence in its support. A case in moths, in a way comparable to that suggested by Winge, had indeed been observed in 1913 by Federley, in Finland, in some hybrids he was studying, but since polyploidy is almost unknown in animals the significance of this example was lost.

Verification for Winge's theory came from several quarters almost simultaneously. In 1925, Clausen and Goodspeed, in the U.S.A., described a case in tobacco in which doubling had occurred under experimental conditions, though it was uncertain how it had occurred. In 1927, Rosenberg, in Sweden, suggested a mechanism by which this doubling might be expected to happen in hybrids ; and his suggestion was soon confirmed by Karpechenko, in Russia, working with the hybrid between radish and cabbage. And finally, an alternative mechanism was found at about this time in *Primula*, at Merton, by Newton and Pellew.

The radish × cabbage hybrid, *Raphanus sativus* × *Brassica oleracea*, will be described first. Both species have 18 chromosomes. They belong to different genera, but can be crossed with little difficulty when the radish is used as female parent though the reciprocal cross, with cabbage as female, cannot be made. The hybrid is intermediate between the two parents, and is almost sterile. The few seeds that are set give, when sown, plants which are like this sterile hybrid in appearance but are fertile and breed true. Their origin was worked out in detail by Karpechenko, who showed that a doubling in chromosome number had occurred. He found that the sterile F_1 had, as expected, 18 chromosomes, 9 from radish and 9 from cabbage. The reduction divisions followed a very irregular course but occasionally gave rise to gametes with 18 chromosomes, and union between two such gametes gave the fertile form with 36 chromosomes.

The origin of these rare 18-chromosome gametes will be considered in more detail. In what may be regarded as the most typical form of the reduction divisions in the sterile diploid, the whole 18 chromosomes remained unpaired or univalent—presumably because of the lack of homology between radish and cabbage chromosomes—and

passed at random to the two poles in the heterotype division. Figure 20 illustrates a case in which 9 chromosomes, some from one parent and some from the other, have passed to each pole; but separation may be into groups of 10 and 8, 11 and 7, and so on.

The nuclei so formed contain a mixture of the very differ-

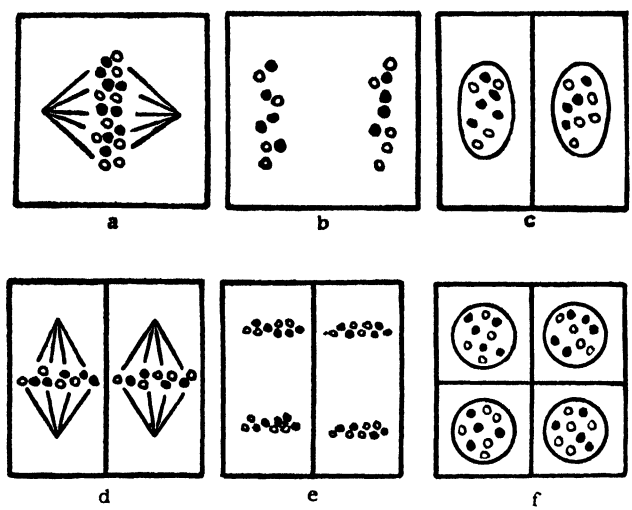


FIG. 20.—Radish × cabbage: the chromosomes segregate at random without pairing, and sterile gametes are formed. *a–c*, heterotype; *d–f*, homotype.

ent chromosomes of radish and cabbage. This mixture is unable to function in the usual way and does not produce fertile gametes; the pollen grains, for example, being aborted and empty, and the plant producing them being sterile. It would be a very rare chance that resulted in all the 9 chromosomes of radish or of cabbage going alone to the same pole and producing a fertile gamete, and it has been found that the few seeds that are formed come through

an occasional modification of the course of the division. In this the 18 chromosomes do not separate properly from one another and all get included in a single nucleus (fig. 21*c*) ; at the homotype division they divide in two as usual, and a regular separation follows (fig. 21*d–f*). Two cells, each containing 18 chromosomes—the complete set, *R*, of 9

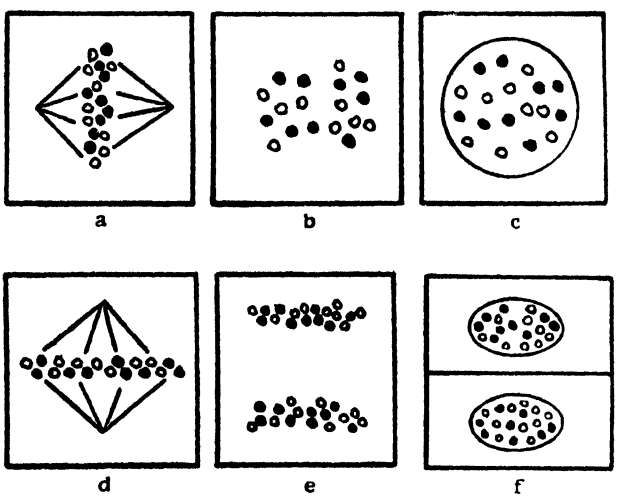


FIG. 21.—Radish × cabbage. The 18 unpaired chromosomes do not separate to opposite poles and are included within a single "restitution" nucleus (*a–c*). The homotype division (*d–f*) gives two cells with 18 chromosomes and these produce fertile gametes.

radish chromosomes plus the complete set, *C*, of 9 cabbage chromosomes—are therefore obtained. These give rise to fertile diploid gametes, *RC* ; and union between two such gametes gives the occasional seed, from which tetraploid plants, *RRCC*, with 36 chromosomes are obtained.

In these tetraploids the reduction divisions are regular. The two sets of radish chromosomes, *R*, and the two sets of cabbage chromosomes, *C*, pair together and give gametes

RC from which tetraploids *RRCC* are once more obtained. The plants are therefore fertile and breed true, although they are almost exactly like the sterile diploid hybrid in appearance.

Another case of hybridization followed by doubling in chromosome number is that of *Primula Kewensis*, a winter flowering plant of some horticultural importance. This plant was first obtained in 1899 at Kew as a hybrid between *P. verticillata* and *P. floribunda*, two well-separated species differing from each other in a number of clearly defined ways. The cross is not easily made but succeeds occasionally with *floribunda* as the female parent. The hybrid was quite sterile and was propagated vegetatively for some years, but in 1905 seed was borne by a plant in the nurseries of Messrs. Veitch. When sown, this seed gave fertile *Kewensis*, exactly like the diploid in appearance ; and from this stock the whole of the modern *P. Kewensis* of commerce has been derived. The sterile diploid has, however, also set seed on two subsequent occasions : at Kew in 1923, and at Merton in 1926.

The latter case was investigated by Newton, who showed that it was an experimental verification of Winge's hypothesis. The two parents, *floribunda* and *verticillata*, were both diploid species with 18 chromosomes ; and the sterile hybrid had, as expected, a like number. The fertile form was found to be a tetraploid with 36 chromosomes ; its origin being due to a somatic doubling, as in the case of the autotetraploid *Datura*, and unlike the tetraploid *Raphanobrassica*. The sterile diploid hybrid at Merton had 18 chromosomes in the vegetative cells of most parts, but produced a single branch with 36 chromosomes. This branch was fully fertile and the seeds gave fertile tetraploids once more. It should be noticed that the two ways in which doubling can occur, that found in *P. Kewensis*, and that

found in *Raphanobrassica*, can easily be distinguished from one another. In the first, an otherwise sterile plant gives a fertile branch; in the second, occasional seeds are found scattered about here and there on an otherwise sterile plant.

The fertile *P. Kewensis*, though more or less intermediate in character between its two parents, is quite distinct from either, and would probably not have been recognized for what it was if its ancestry had not been known. Attempts to cross it with its parents have so far failed. For these reasons, and from the fact that it breeds true, it may justly claim to be looked upon as a new species artificially produced.

It is only natural to suppose that many of the polyploid species found in nature have arisen in a way similar to *Raphanobrassica* or *P. Kewensis*. Various factors may make it difficult to get direct evidence about the origin of an existing form, but the indirect evidence is often strong. One such case is that of the grass *Spartina Townsendii*, which is found on muddy foreshores in harbours and estuaries, and has been planted extensively in recent years in Holland to reclaim land from the sea, since it effectively prevents any mud deposited from being washed away. In this use it is valuable, though its spread in harbours may easily become a nuisance. The species was not known before 1870, when it was discovered in Southampton Water, and it is believed now to have originated as a hybrid between *S. stricta* ($2n = 56$) and *S. alterniflora* ($2n = 70$). The first of these is a European species; the second almost certainly an American one, which appears to have been accidentally introduced to Europe by shipping, and was first recorded in 1803 at Bayonne in the Bay of Biscay, and near Southampton in 1829. It was long ago suggested that *S. Townsendii* was a hybrid between these two species, partly because it

is more or less intermediate between them in appearance, and partly because it was found in the only two places in the world where *stricta* and *alterniflora* meet. Chromosome counts confirm this conclusion; for Huskins, in Canada, has shown that *S. Townsendii* has 126 chromosomes, the number to be expected if doubling had occurred in a hybrid between *stricta* and *alterniflora*. The new species is said to breed true, but may perhaps do so only approximately. It spread slowly on its first appearance, but very rapidly later; so that thousands of acres on the south coast of England were covered by 1907. It crossed the English Channel in 1906 and now covers large areas on the north coast of France. Wherever it has come in contact with *S. stricta*, one of its parents, the latter seems to have been practically exterminated. This is clearly important since it suggests that polyploid forms may often replace one or both of their parent species, with the result that the exact manner of their origin could never be demonstrated.

Very definite evidence of origin by hybridization has also been found in the genus *Galeopsis* by Müntzing in Sweden. He has obtained a form that is almost identical with the tetraploid species *Tetrahit*, hemp-nettle, from a cross between two diploid species of the genus, *speciosa* and *pubescens*. These two give a partially fertile hybrid. The artificial *Tetrahit* was not obtained directly from this hybrid by a doubling in chromosome number, but indirectly from a back cross between the F_1 and the *pubescens* parent. However, the exact manner of its origin need not concern us here: the important feature is that the new form, artificial *Tetrahit*, appears to be almost identical with the wild, and crosses readily with it to give a fully fertile hybrid. So far as we can tell, a species of wild plant has been artificially synthesized from two other wild species.

In the polyploid wheats, *Triticum*, there is a strong

probability that the tetraploids and hexaploids arose by hybridization followed by chromosome doubling, though there is not a great deal of evidence to indicate the parent forms. If, however, the relationship between the tetraploids and the hexaploids had been an autopolyploid one—that is, if they contained exactly the same set of chromosomes, in the one case four times and in the other six—some of the hexaploids might reasonably have been expected to resemble closely some of the tetraploids, and the latter to resemble the diploids. Actually, this does not occur. It could be supposed that the hexaploids have changed since their first formation, but even then some of them should have their prototypes among the tetraploids and they do not. Instead, every hexaploid has some characters that are not found at all in the tetraploids or are developed to a different extent, exactly as would be expected if a series of characters had been introduced by hybridization with a distinct species.

It may be supposed that a tetraploid would have come from a cross between two diploid species, of which one carried in its germ-cells a set of 7 chromosomes that may be denoted A_1, and the other a somewhat similar set denoted by A_2. These two would give a sterile hybrid A_1A_2, from which a fertile tetraploid, A_1A_1 A_2A_2, arose. In a similar way a hexaploid could be derived from three sets A_1', A_2', and A_3'. The difference in appearance between the two series, that is between A_1A_2 and $A_1'A_2'A_3'$, may be due partly to differences between A_1 and A_1' or A_2 and A_2', and partly to the further possession by the hexaploid of characters associated with A_3'.

These examples make it probable that the allopolyploid series so often found in Nature, especially among flowering plants, have originated by hybridization followed by chromosome doubling, like the *P. Kewensis* produced under experimental conditions. Their origin explains why the different

members of a series usually differ from one another in characteristic ways ; unlike autopolyploids, which are almost identical in appearance with the diploids from which they came.

(References are given at the end of Chapter IX.)

CHAPTER IX

CHROMOSOME VARIATION (*continued*)

NOW that the origin of polyploid series has been described it is natural to ask what happens when forms with different numbers are crossed. Whether forms with new intermediate numbers are obtained, what kind of segregation occurs, and so forth.

As might be expected, there are many irregularities, both genetical and cytological, in crosses of this kind ; and these irregularities have led to important genetical conclusions. The first case to be studied was a cross between two species of sundew, *Drosera*. The Swedish botanist, Rosenberg, found that when *D. rotundifolia* ($n = 10$) and *D. longifolia* ($n = 20$) are crossed, 20 of the 30 chromosomes in the F_1 pair together at the heterotype division to form 10 bivalents, the remaining 10 being left as unpaired, univalent, chromosomes.

A more detailed description was given later by the Japanese cytologist Kihara, for the pentaploid wheat hybrids ($2n = 35$) formed by crossing various hexaploids ($n = 21$) with different members of the tetraploid series ($n = 14$). This cross has now been thoroughly analysed, but since it is complicated it will be better to describe first the behaviour of certain types in *Datura*, which have been studied in some detail in recent years by Belling, Blakeslee, and their co-workers in the U.S.A.

The Jimson Weed or thorn-apple, *Datura stramonium*,

belongs, like the potato and the tomato, to the family Solanaceae. Under experimental conditions it has given rise to a number of new forms of various kinds. One series has one more than the usual number of chromosomes, 25 instead of 24. These forms arise from an occasional irregularity in the reduction divisions, which results in the two members of a pair of chromosomes going both to the same pole instead of to opposite poles. When this happens, instead of a gamete containing 12 chromosomes, *ABC . . . L*, one with 13, *ABC . . . L + A*, is produced; and this, uniting with a normal gamete, gives a 25-chromosome plant, *AA BB CC . . LL + A*, containing the *A* chromosome three times and every other chromosome only twice. This plant is said to be a *trisomic*.

In the case described 12 different trisomics are possible, by the duplication of any one of the 12 members of the original set; and it is claimed that all these have been found in practice. The effect of an extra chromosome such as this on development is usually seen in small changes in all parts of the plant, and in a reduction in fertility. These changes, it should be noticed, are brought about not by the introduction of entirely new genetical material, but simply by the fact that some of the material is present three times and the rest only twice.

The transmission of the extra chromosome in a trisomic will be described more fully; the one that produces the type of plant known as "Globe" being taken as illustration.

In the reduction divisions of a trisomic *Datura* the three homologous chromosomes may either all unite to give a trivalent, or two of them form a bivalent while the third is left as a univalent. Usually, the paired chromosomes behave normally both in the heterotype and in the homotype division, and the extra chromosome segregates at random to either pole in one division and splits longitudinally in the

other (fig. 22). As a result of this behaviour the extra chromosome is transmitted to about half the gametes, cells

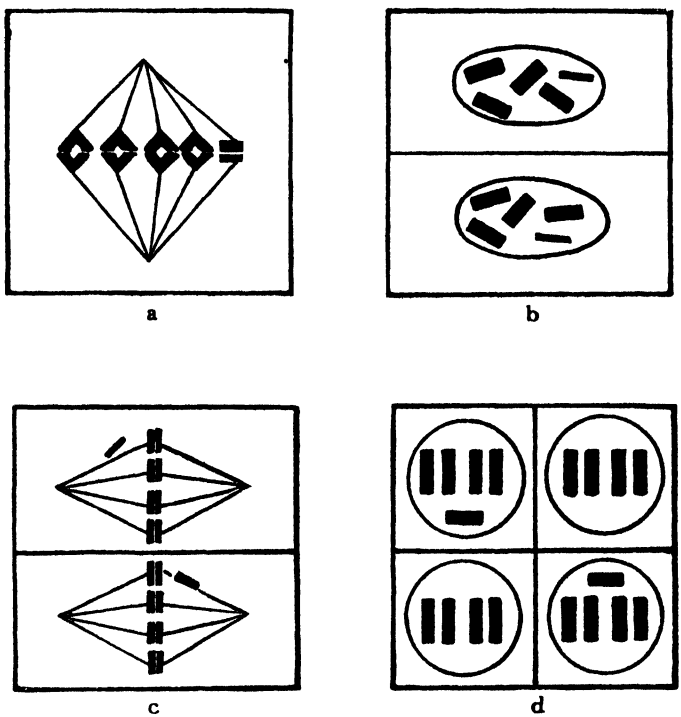


FIG. 22.—Organism with 4 bivalent plus 1 univalent chromosomes.

Diagram showing behaviour of univalent at meiosis; at the heterotype (*a*, *b*) it splits longitudinally and at the homotype (*c*, *d*) it segregates to either pole so that cells with either 4 or 5 chromosomes are produced.

containing either 12 or 13 chromosomes respectively being each formed in about 50 per cent. of cases.

Denoting the complete haploid set of 12 *Datura* chromosomes—*ABC* . . . *L*—by *X*, and supposing that it is the *A* chromosome that is responsible for producing "Globe,"

then meiosis in Globe produces cells containing respectively X and $X + A$ in approximately equal numbers. If these cells formed gametes that mated at random the result would be, as in ordinary Mendelian inheritance, a ratio of 1 : 2 : 1 for the three types XX, $XX + A$, and $XX + AA$, with 24, 25 and 26 chromosomes respectively. It is found, however, that although these three types are produced, as expected, the ratio of 1 : 2 : 1 is very far from being realized. The observed ratio varies according to the particular chromosome concerned and with other circumstances; but the general tendency is for the normal type, XX, with 24 chromosomes, to be more numerous than expected, and for the so-called tetrasomic type, $XX + AA$, with 26 chromosomes, to be rare.

It has been shown that this result is associated with the reduced fertility of the parental 25-chromosome plant, especially that of the pollen. By careful studies of the growth of the pollen tubes, Buchholz and Blakeslee have shown that, although both 12 and 13 chromosome pollen can function, the latter produces pollen tubes that grow more slowly and frequently burst, and are not nearly so successful in fertilization as those from the former. Supposing it to be 10 times more likely that fertilization will be accomplished by a 12- than by a 13-chromosome male gamete, then the zygotes formed would be

$$(1X + 1\overbrace{X + A}) (10X + 1\overbrace{X + A}) = 10XX + 11\overbrace{XX + A} + 1\overbrace{XX + AA}$$

In other words, normal diploids and trisomics would be produced in about equal numbers, together with a small number, about 5 per cent., of tetrasomics.

In the wheat hybrids it has been shown that the exact proportions in which three types such as these are found is also influenced by other factors, such as a difference in

the percentage of germination of the different seeds. These may be neglected at present.

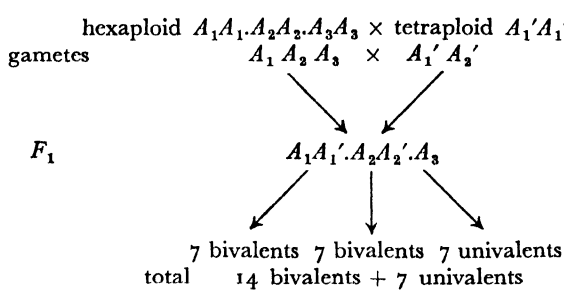
The tetrasomics have in an exaggerated degree, as would be expected, the characteristic features of the trisomics from which they came. They are also very weak and slow growing, and are usually highly sterile. Evidently, too great a change in the number of *A* chromosomes, with no corresponding change in the others, has a profound effect upon the whole physiology. The results may be taken as typical of the other tetrasomic forms in *Datura*, and of those found in some other plants, such as *Œnothera Lamarckiana* or evening primrose, *Solanum lycopersicum*, the tomato, and *Matthiola incana*, the garden stock. In *Drosophila melanogaster* tetrasomics do not live.

These *Datura* forms illustrate some important features. In the first place, the reduplication of a chromosome has a marked effect on the development of an organism. Secondly, gametes that carry the extra chromosome may be less effective than those that do not. Finally, in consequence of this, $2n + 1$ forms rarely give $2n + 2$ segregates, and there is a general tendency to revert to the number $2n$.

It may be stated also that $2n - 1$ forms (forms with one chromosome less than the normal number, which may be produced under certain circumstances) would also, in a similar way, tend to give $2n$ progeny; though the tendency is often less pronounced.

This brief, and somewhat simplified, account of the genetics of trisomics in *Datura* makes it easier to understand the more complicated case of hybrids between different members of a polyploid series. Here also, as shown in detail in the polyploid wheats, reversion to the normal number of chromosomes is a conspicuous feature. The reasons for this reversion, and how it tends to keep the original forms separate, will now be described.

The hexaploid wheats ($n = 21$), with three sets of 7 chromosomes, $A_1A_2A_3$, in the gametes, are easily crossed with the tetraploid ($n = 14$), with two sets A_1' A_2', giving the pentaploid hybrid ($2n = 35$) A_1A_1' A_2A_2' A_3. In this hybrid the chromosomes behave like those in the *Drosera* hybrid. Fourteen bivalents are formed by the pairing of A_1 and A_2 with A_1' and A_2', and 7 chromosomes, the set A_3, are left as univalents.



The bivalents behave normally, 14 chromosomes travelling to opposite poles of the cell, but the univalents are distributed irregularly, any number from 0 to 7 being included in the daughter nuclei, so that the cells from which the pollen grains and egg-cells are produced may contain any number of chromosomes from 14 to 21. In F_2 it is found that mating between these gametes has given zygotes with any number of chromosomes from 28 to 42, as would be expected.

Further breeding reveals the important fact that, although the plants with 28 and 42 chromosomes have regular reduction divisions and breed true to chromosome number, the plants with an intermediate number are none of them fully fertile, and their progeny show a general tendency to revert to one of the two parental numbers; so that, if breeding is continued, only plants with these numbers, 28 or 42,

are obtained. In principle, the reasons for this behaviour are the same as those responsible for the trisomic *Datura's* giving chiefly normal type offspring. In the first place, pollen with 14 or 21 chromosomes functions more readily than that with intermediate numbers ; and secondly, forms corresponding to the tetrasomics in *Datura*, which might have been expected to breed true to an intermediate chromosome number, are, as in *Datura*, weak and sterile. It has been shown that these two facts are responsible for the main features of the genetical behaviour, but the number of univalents involved—7—makes the working out complicated.

These results show how the different members of a polyploid series may remain sharply separated from one another even when they are able to cross. The members of an autopolyploid series are in any case similar to one another. Allopolyploids usually show marked differences ; though since there may be all degrees between autopolyploidy and allopolyploidy no hard and fast rule can be laid down. The important point to notice, however, and this arises clearly from the results just described, is that two allopolyploid forms which are already distinct will not be likely to give intermediate forms as the result of crossing.

This is so for several reasons. In the first place, the hybrids are partially sterile and will leave fewer offspring than the parents. Secondly, they do not usually give stable segregates with new chromosome numbers, at any rate in wheat, and this probably applies to many other genera, though reasons will be given for thinking it may not apply to all. It only remains to show, thirdly, that the fertile tetraploid and hexaploid segregates which are obtained differ markedly in their characteristic features, as did the original parents.

This latter phenomenon has been worked out genetically in the case of the wheat hybrids. In individual cases the

details are complicated, but the fundamental principle is not difficult to understand, being connected with the additional chromosomes of the hexaploid parent, which remain unpaired at the reduction divisions. It will be recalled that the hexaploid wheats, derived from three different diploid forms, and therefore having three sets of seven chromosomes each in the gametes, may be given the gametic formula $A_1A_2A_3$. The tetraploid may similarly be called $A_1'A_2'$, and the pentaploid hybrid would then be $A_1A_1'.A_2A_2'.A_3$. In this hybrid, segregation occurs as usual between A_1 and A_1', or A_2 and A_2', so that some characters are transferred from one species to the other by crossing. Examples are rough and smooth chaff, bearded and beardless ears, and many others. Tetraploid and hexaploid will still remain distinct, however, because of the effect of the chromosomes, A_3.

Tetraploid × hexaploid

$A_1'\ A_2'\ \times\ A_1\ A_2\ A_3$

pentaploid F_1 $A_1A_1'.A_2A_2'.A_3$

tetraploid segregates $A_1A_1.A_2A_2'$, $A_1A_1'.A_2A_2'$, $A_1'A_1'.A_2'\ A_2'$, etc.

hexaploid segregates the same, with the addition of A_3A_3, e.g.

$A_1\ A_1.A_2'\ A_2'.A_3\ A_3$

In the first place, if these chromosomes carry factors not found in the other sets they will give characteristic features to the hexaploids and to these only. Secondly, it has been shown that since a combination such as $A_1A_2A_3$ could never occur in the tetraploid it is commonly found that the hexaploid can be recognized by having a combination of characters that does not occur elsewhere.

For these various reasons it is easily possible to separate the tetraploid and hexaploid segregates by their appearance. The two parental species, therefore, even if they do cross with one another, will remain distinct. Some transference of characters can occur, and forms less distinct than the

original species can be obtained, but this occurs only to a limited extent—it still remains true that groups of forms distinguished with more or less ease by their appearance as well as their chromosome number are produced.

The question arises how far the results found to hold in the case of wheat are likely to apply elsewhere. There are certainly many genera that show the same behaviour, but it should hardly be expected that the experience would be universal. The reason for the absence of intermediates is the weakness and sterility of forms in which some only of the chromosomes in a set are duplicated. This weakness and sterility might not be invariable ; and there are indeed reasons for thinking that in some genera duplication of chromosomes can occur without this accompaniment. The duplication need not necessarily be the result of hybridization ; it might conceivably arise in some other way.

An interesting example is the genus *Viola*, to which the pansy belongs. This genus is almost world-wide in its distribution. It contains some 500 or so species, with the gametic chromosome numbers varying in irregular fashion from 6 to 36, thus : 6, 9, 10, 11, 12, 13, 15, 17, 18, 21, 24, 26, 27, 30, 36, have all been reported. There are grounds for believing that the basic number of the genus is 6, and that forms with 6, 12, 18, etc. chromosomes should be regarded, at least in some cases, as the terms of a polyploid series. If this be so, it is natural to ask whether the contrast between the series of numbers in *Viola* and the regular polyploid series found in wheat may be due to the fact that reduplication of one or more chromosomes is less likely to cause weakness and sterility in *Viola* than in wheat. Thus the numbers 13 and 17 might perhaps have been produced from 12 and 18 by the addition or loss of one chromosome of the set. Some support to this possibility is given by the results of crossing the two species *tricolor* ($n = 13$) and

arvensis ($n = 17$). Although generally speaking the behaviour of this cross is not very different from that of the wheat hybrids, there is evidence that, occasionally, true breeding forms with a chromosome number intermediate between those of the parents may be obtained.

There are other cases in which it has been suggested that an irregular series of numbers may have arisen by the duplication of one or more of the chromosomes in a set. One of the first genera in which evidence was obtained is *Dahlia*; but a more interesting example is found in the family Rosaceae. This is a large and variable family of flowering plants in which the three most important groups are the Rosoideae (including the rose, *Rosa*) with 7, the Prunoideae (including the plums and cherries, *Prunus*) with 8, and the Pomoideae (including apples and pears, *Pyrus*) with 17 as the basic chromosome numbers. Darlington and Moffett suggest that 7 is the basic number for the family, and that the number 17 should be regarded as $7 + 7 + 3$. Representing the 7 original chromosomes in the family by the letters *A B C D E F G*, then the set of 17 would be represented by

$$A_1B_1C_1D_1E_1F_1G_1$$
$$A_2B_2C_2D_2E_2F_2G_2$$
$$A_3B_3C_3$$

where A_1, A_2 and A_3, B_1, B_2 and B_3, and so on, are similar but not identical.

The evidence for this view is the behaviour of the chromosomes in the reduction divisions. It is there found that instead of the chromosomes only occurring in pairs, as they do in a true diploid, they may be associated either in sixes—the three *A*, *B*, or *C* chromosomes and their homologues—or fours—the *D*, *E*, *F* and *G* chromosomes and their homologues. The exact nature of the associations are uncertain, however,

and nothing is known about the important question how similar the different sets are, or about the nature of the differences between them.

Although it is likely that reduplication of whole chromosomes is sometimes the reason for irregular differences in chromosome number, very little is known yet about the

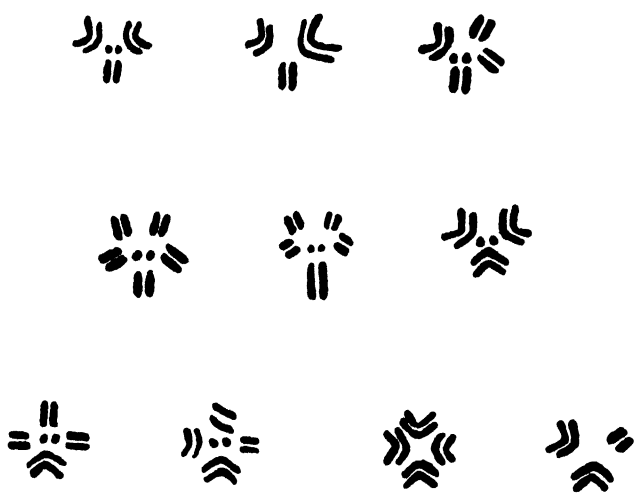


FIG. 23.—Chromosomes of some different species of *Drosophila* × 3,500 (after Metz and Moses).

exact nature of the genetical changes concerned in any one case; there can be no doubt that entirely different processes—such as fragmentation, or the breaking of one chromosome into two or more parts—must be reckoned with, and sometimes the change may well be of a complicated kind. It is only recently that the study of irregular series has begun.

Interesting differences occur in *Drosophila*, many of the

species being recognizable by their chromosomes (fig. 23). The species of this genus that have so far been collected are very definite and clearly separated from one another but, unfortunately, cannot be crossed, except in the case of the closely similar *melanogaster* and *simulans*, which give a complete sterile hybrid.

In the genus *Crepis*, a flowering plant belonging to the dandelion family Compositae, and the subject of study by Babcock and his fellow-workers in California, the gametic numbers 3, 4, 5, 6, 7, 11, 20, amongst others, occur in the different species. The chromosomes (fig. 24) show considerable variation in size, in the relative lengths of the two arms, and in the possession of small bodies known as satellites or trabants. Many of the species will cross with one another, so it may be hoped that they will teach us how irregular series of this kind arise.

It will be seen that in the case of polyploid series there is definite information both about their origin and about the behaviour of hybrids between the different members. Far less is known about either the origin or the genetical behaviour of other numerical series ; but, probably, some of the cases are associated with the duplication either of whole chromosomes or of parts of chromosomes. It is noteworthy that, from the evidence of forms having a single extra chromosome, and similar forms, this kind of change will usually differ from single factor changes in a tendency to affect many characters a little, instead of one or a few characters a great deal.

Any change in number of chromosomes will tend, as with polyploidy, to keep different types distinct. This feature is important. When two types differ in Mendelian factors only, even when the differences are considerable, crossing will break down the distinctions between them by giving individuals in which the differences are found in

FIG. 24.—Chromosomes of some different species of *Crepis* × 2,500 (after Hollingshead).

every possible combination. When a difference in type is associated with a difference in chromosome number this breaking down is not likely to occur; in addition the hybrids will show some degree of infertility. This latter kind of variation will therefore often be associated with a separation of individuals into distinct groups; and it has been found that these are sometimes of the same kind as the groups recognized by the systematist.

Reduplication of chromosomes, especially in polyploids where it has been chiefly worked out, and to a less extent in irregular series, has now been dealt with in some detail. It is one way in which variation in the chromosome complement may occur. A second way was described in an earlier chapter where it was shown, partly by linkage data and partly from observations on chromosome pairing, that there might be variation in the arrangement of the genetical factors in the chromosomes. By the combination of these two mechanisms—reduplication and reorganization—there may be great variation in the chromosome complement; and observations on the reduction divisions in species hybrids show that these possibilities are realized in practice. The results may be complicated, but analysis is assisted by the theory that pairing at reduction occurs only between homologous particles, and it may be hoped that with this help a proper study of the different variations will prove possible. At present, though there is much more to be learned about these kinds of variation, their general importance is sufficiently shown by the cases described, which make clear the special features that will be found in hybrids between forms differing in these respects.

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CHAPTER X

MUTATION

ANY theory of evolution must suggest a means by which new variations can arise.

To Darwin, this gave no special difficulty. He held that variability would be provoked by changed conditions : that a species when first domesticated, for example, would be specially prone to give new varieties because of the new conditions under which it lived. But he did not pay great attention to the matter because, although little was then known about the subject, there was no disputing the fact that variation occurred. It was easy to see that the members of the same species were not all alike, and that offspring were not an exact copy of their parents. The actual nature of the differences could, he thought, be disregarded ; because whatever their nature, the selection of one type instead of another would bring permanent change.

Today, with the knowledge gained by genetics, we know that Darwin's view was wrong. Selection does not always bring permanent change. It does not do so in the case of the pure line. According to the teaching of genetics, when individuals differ they differ in constant units, which are transmitted unchanged from one generation to the next ; and, once the most favourable combination of units has been selected, no further change would be possible. Were selection carried out in man for tallness, it would be expected that a taller race would be produced, with an average height,

perhaps, of 6 feet 6 inches instead of 5 feet 6 inches. But the change would not go on indefinitely. Because the original race varied, say, from 4 feet 6 inches to 6 feet 6 inches, it does not follow that selecting the tallest would give a range from 5 feet 6 inches to 7 feet 6 inches, and so permit still further change. Darwin and his followers overlooked the fact that selection diminishes variability; and that having selected the tallest the new race would be comparatively uniform instead of highly variable like the old.

The origin of new variations has, therefore, become an important question. If the hereditary units never changed there could be no evolution; and it is in fact believed that new forms come by mutation, a sudden change by which a factor *A*, for example, may be converted to its allelomorph *a*. In this way a homozygous race, *AA*, with purple flowers would give a single heterozygote, *Aa*, from which the recessive white, *aa*, would be segregated and would appear as one or more new mutants.

Mutation is a rather rare event, and little is known about the circumstances that bring it about; but there is no doubt that new mutants do sometimes arise. They have often been observed during genetical experiments; and have long been known to plant and animal breeders, usually under the name of "sports."

Sometimes the occurrence of mutation can be inferred when an attempt is made to trace the origin of the present-day varieties of cultivated plants. When a species is first introduced to cultivation, variation may be introduced by deliberate crossing with other species; but in other cases the records existing suggest that new forms have arisen suddenly as single individuals and not as the product of crosses. A good example is the sweet pea, *Lathyrus odoratus*, which was first introduced to England in 1669, when a few seeds were imported from Sicily. Hybridization was not

practised until 1880, but several new flower types had appeared spontaneously before that date ; scarlet colour, for example, in 1793, and picotee in 1860. The sweet pea will not hybridize with other species. Other plants provide similar illustrations.

Examples of factor mutation are also given by the so-called bud " sports," which are well known in horticultural and genetical literature. In these cases a plant of one variety will give a single branch having the characteristic features of another ; the peach, as Darwin noted, has often been reported to produce a branch bearing nectarines. Such an occurrence arises when, during growth, a bud has arisen from a single mutated cell. In these circumstances a whole branch will be affected. In other cases the tissue concerned may be only a single flower or fruit, or even part of a fruit ; occasionally, for example, an orange is found with one sector of the skin yellow in colour instead of orange, the two areas being sharply set apart from one another.

Similar cases are found in animals. In *Drosophila melanogaster* a male fly with one red and one white eye has been found in a red-eyed stock, showing that during development a mutation from red to white had occurred in the *X* chromosome. Some of the germ-cells were descended from the mutated cell, so that the white factor was transmitted to the progeny of this abnormal male, as breeding tests showed. Cases of this kind are common in Lepidoptera.

Excepting lethal mutations, which are often dominant, most mutant forms are recessive to the normal, and are therefore not likely to be observed until one or more generations after the mutation occurred. Thus, should a mutation arise in a germ-cell, it will produce a gamete carrying *a* instead of *A* ; there being only one chromosome of each kind present, so long as the type form is a normal diploid and not a polyploid. Unless many mutant germ-cells are

produced at the same time, the new *a* germ-cell is almost certain to unite with the normal *A* germ-cell, giving a heterozygote *Aa*. In a self-fertilized plant this would give the recessive form in the next generation ; in animals with separate sexes, or in cross-fertilized plants, several generations might elapse before the new mutant revealed itself.

Similar reasoning applies to cases of somatic mutation. In somatic cells there are two chromosomes of each kind, and mutation would only affect one of them, giving, in a homozygous individual, *AA*, a heterozygous portion, *Aa*, which would escape unnoticed except in the case of a self-fertilized plant or if dominance were incomplete. In a heterozygote, *Aa*, somatic mutation can give a visibly different portion, *aa* ; but in cases of this kind it may be uncertain whether mutation was responsible, since the production of an *aa* cell by a heterozygote *Aa* might conceivably be the result of some kind of segregation in a somatic cell, caused by an irregular mitosis perhaps. It will therefore be appreciated that when a new form appears the evidence may need to be considered carefully before this new form can be accepted as a mutation. The most convincing cases are probably given when a new type arises in a stock that has bred true for many generations, as in *Drosophila* and *Primula*.

Discussion of mutation will be restricted here to changes in the nature of the genetic factors ; and not applied to changes brought about by an irregularity in chromosome behaviour, such as the production of forms with an extra chromosome, polyploid forms, or forms that have lost a portion of a chromosome, all of which are changes of a totally different kind. Disregarding these cytological changes, therefore, the observed cases of mutation have been due to a change in a single factor, not in several factors at once. In *Drosophila* it was estimated in 1925

that as many as twenty million flies had been examined in the course of experimental work, and among these several hundred different mutations had been found; the most frequent being that from red to white eye, which had been recorded 25 times.

It is now generally admitted that mutations do occur from time to time, and further inquiry has been directed partly to finding out the causes that produce them, and partly to observing the nature of the changes brought about —particularly whether they are of a kind that agrees with views on evolutionary progress.

The last of these questions is very important; for it is clear that on genetical principles, as so far understood, evolution could not have occurred without mutation, which provides the only means of escape from the pure line; and critics who do not admit the importance of genetics as a method of studying evolution have denied that the mutations so far observed are likely to have any evolutionary significance.

Discussion will have to be based chiefly upon the *Drosophila* results, since there is no other organism in which mutation has been so comprehensively studied. It is therefore doubtful how widely the conclusions can be applied; but so far as can be told from the occasional mutants found in many other organisms, the *Drosophila* experience will prove to be common.

Dominant mutations are usually, but possibly not always, lethal when homozygous, like the factor *D* for Dexter cattle. Many of them have been shown to be due to a deficiency, that is to say, to the loss or inactivation of a portion of one of the chromosomes.

Disregarding these cases most other mutants are recessive to the normal, and have also been represented as having no evolutionary importance. Like most recessives they can

be described as having lost some potentiality possessed by the normal type—with eyes, for example, that are colourless instead of coloured—or they are characterized by some abnormality, such as vestigial wings, which would handicap them severely in the struggle for existence. Indeed, the homozygous recessive, the mutant form, is usually less vigorous than the normal type and cannot compete with it.

These facts are important in themselves. They have an added importance because they have been represented as showing that mutations are merely retrograde changes, or losses. The argument derives some of its support from the presence and absence theory of Bateson and Punnett, according to which the recessive would arise by the loss of something present in the dominant. It is also worthy of consideration because it provides an explanation for certain undoubted facts for which an explanation must be found. In the first place it is usually true that the loss of a potentiality, lack of colour, lack of hair, and so on, is inherited as a recessive. Secondly, most mutants are recessives; many are abnormalities; and most are weaker than the normal type; all of which we should expect if they are the result of loss of some kind. It would no doubt be wrong to look upon all changes of the kind described as the result of complete loss, but the facts undoubtedly suggest something that may be described as degradation, or the passage from the complex to the more simple.

The conception also receives support in some cases, as argued in an earlier chapter, from the existence of multiple allelomorph series. These series of factors usually affect the same character, but affect it with varying intensity, as in the case of the series that modify the size of the yellow eye of the flower in *Primula sinensis*. Whenever they behave in this way they are most easily thought of, agreeably with the presence and absence hypothesis, as being due to a

graded series of changes in the factor concerned. For all these reasons it is easy to argue that the great majority of mutations are recessive losses, or cases of even more serious damage, that reveal themselves as dominant lethals; and this would make it doubtful whether mutation has any fundamental importance.

It must be admitted that this criticism contains much truth; but from what is now known it appears to be too simple an interpretation and one not always valid. A proper consideration of the question is impossible, since we know nothing whatever about the physical or chemical nature of genetical factors; and this makes any discussion of their innate character seem intangible. If we mean by presence and absence only that the allelomorphs *A* and *a* are related to each other as the complex is to the more simple, then probably many mutations are usefully described as losses. But the chemical nature of the genetical factor, whatever it may be, is likely to be complicated; and it is probably also safe to suppose that the possible changes in these factors—mutations—are many and various in kind; and not all losses, even if some of them are.

This view of factor mutation agrees with what has been seen in *Drosophila* and other organisms. Though little can yet be attempted in the way of classification, what can be done leaves no doubt that different kinds of change do occur.

This has been clearly shown in the case of multiple allelomorphs. For although multiple allelomorphs often have effects that can be arranged in a graded series, agreeing with the supposition of quantitative changes in the factor, in other cases their effects cannot be so arranged, and qualitative, not quantitative, changes must be assumed. The supposition of a quantitative change was clearly shown by Stern to hold good for the four allelomorphs belonging

to the series known as "bobbed." These factors have the effect of shortening the bristles in *Drosophila* by varying amounts. They are carried by the *Y* chromosome; and by using races with varying numbers of *Y* chromosomes he was able to show that an increase in bristle length could be brought about either by increasing the number of short-bristle factors, or by substituting a factor higher in the series; though it was not possible to increase the length above that caused by the "normal" factor.

Other cases are known, however, in which it can be shown that there is not a simple quantitative relation between the members of a series. This is so, for example, in the series in *Drosophila* known as "stubble," as Dobzhansky, working in U.S.A., has shown. The factors in this series —stubble, stubbloid, and normal—affect several characters. But there is no order in which the factors can be arranged to agree with the idea of a quantitative series; for some characters are more affected by stubble than by stubbloid, while in others the reverse is the case. It seems here that the change from normal to stubble must be of a different kind from that of normal to stubbloid: quality, not quantity, is in question.

Another example is given by the blood groups in man. Human beings can be divided into four groups, according to whether their blood will or will not coagulate when mixed with blood from members of other groups. Genetically, the difference between these four groups is controlled by a series of multiple allelomorphs, which determine the production of different kinds of substance, not of different quantities, in the blood.

This evidence shows that although some series of multiple allelomorphs, as in the case of bobbed, are best described as due to quantitative changes—successive stages in the loss of a factor—others cannot; but must have been produced

by qualitative changes. Factor mutations, therefore, are of different kinds, and cannot always be losses.

It must also be realized that although many mutant forms in *Drosophila* and in other organisms are less vigorous than the normal form, it should not, therefore, be assumed that all are. Judging from the *Drosophila* experience, a minority of mutants equal the normal type in viability under experimental conditions of culture, though it has not yet been shown that the same would be the case under natural conditions.

It was found in 1927 by Muller, at Texas University, that the rate at which mutations occur can be very greatly increased in *Drosophila* by subjecting the flies to the action of X-rays; and the discovery has since been extended to maize and other organisms. In *Drosophila*, either the larvæ, or the adult flies themselves, are irradiated with the appropriate dose. In adult flies mutations may then be produced directly in the germ-cells; in larvæ, cells in the germ track may be affected and the mutation will be transmitted to all the germ-cells derived from the affected cell. In maize, Stadler, working in U.S.A., has shown that mutation may occur when the pollen is irradiated. In either case the mutant types will be brought to light by mating the treated individuals with each other. Pollen, for example, carrying the mutant factor *a*, mating with normal egg-cells would give heterozygotes *Aa*. These, mating among themselves, would give the mutant form *aa*.

No doubt the supply of mutants in most of the organisms used for genetical work will be greatly increased by the use of the method. In *Drosophila* the mutation rate has been increased some 150-fold, and besides the mutations already known new ones have been produced. So far, X-rays seem to be the most effective agent for producing mutations; though others, such as heat, have been used success-

fully under some circumstances. In several species of moth, Harrison, at Durham University, believed it was possible to induce melanic mutations by introducing small quantities of the salts of lead and manganese into the food; but McKenny Hughes, at the John Innes Horticultural Institution, has repeated the experiment on a larger scale with only a negative result, so that the precise conditions needed to produce these mutants are still a matter of doubt.

An important result obtained from X-ray work is that reverse changes, from mutant back to normal, can be obtained. This was first suggested by Timoféëff-Ressovsky, working in Moscow with *Drosophila funebris*. He obtained a recessive mutant in which the end of one of the veins on the wings was absent; and there was strong evidence that in 1925, in a culture homozygous for this character, a reverse mutation to normal had arisen. Since that date he has employed X-rays to prove the occurrence of reverse mutations, and extensive experiments have also been carried out in *D. melanogaster* by Muller with the same object.

Muller showed clearly that mutations to normal were obtained when flies breeding true to the mutant character "forked" were irradiated; and from the stock of normal flies so produced the forked mutation was obtained once again by X-raying. Such cases are important because they show that the genetical effect of X-rays cannot always be dismissed as examples of irreversible damage done to the chromosomes; though it is possible that some cases can be so described. Reverse mutations from some factors, however, are far less frequent than that from forked. The change from white to red eye must be very rare, and it is even doubtful whether a true mutation from white to normal red eye has occurred; though the change from red to white is one of the most frequent, whether under X-ray or ordinary experimental conditions. A mutation from white to eosin

—another allelomorph in the series—has been reported by Morgan to have occurred in a pure white stock, but not the change from white to red. A somatic mutation from white to red has also been reported, a white-eyed fly having been found with one or more red facets to the eye ; but an element of doubt must remain in cases of this kind since it is naturally impossible to subject the alleged mutation to a breeding test, which alone will establish the genetical nature of the change. Altogether, the somewhat meagre results available suggest that reverse mutation is much more likely to occur in some mutants than in others ; and it is quite likely that some mutant changes may prove to be irreversible.

The evidence discussed leaves no doubt that factor mutations do arise, and that the frequency of the process can be influenced by physical agencies. However, though different kinds of change may certainly occur, it seems that many, possibly most, of them are retrogressive ; and may sometimes be represented as actual losses. For this reason, though all mutations are not of this character, it is difficult to affirm categorically that evolution can be explained on the basis of the mutations that have so far been observed. This conclusion is certainly unsatisfactory ; and that so little should be known about mutation is perhaps the least satisfactory feature of modern genetics. At present, it will suffice to point out two possible explanations of the position.

In the first place, although nothing definite is known about the nature of the changes that occur in mutation, it will be agreed that destructive, retrogressive, changes might easily be more frequent than those that are constructive or progressive. This might be especially true for the organisms chiefly used in genetical experiments, since these are nearly all higher organisms that have already gone far

along the evolutionary paths. It is not necessarily surprising that progressive mutations have not occurred in the period over which observations have been made, which is almost negligible in terms of evolutionary time.

A second possibility, developed especially by Fisher, at London University, is in some ways rather similar; but expresses the matter more definitely by assuming that mutation will be at " random "—by chance in any direction. Put in its simplest terms, the argument is that every organism is highly adapted to its environment, so that any marked change will probably be for the worse. This view may have some truth, and it certainly emphasizes the fact that whatever may cause mutations does not seem to make them in any way purposeful, or adaptive. Assuming randomness, it is possible to treat mutation statistically, and Fisher has accordingly developed a mathematical theory of natural selection. There is no doubt that for evolution to be properly understood the effects of selection will have to be treated mathematically, but in this first attempt the assumptions that have to be made, especially that of random mutation, are such that the theory cannot yet be applied generally. One aspect of the matter must be considered, however, since it gives a somewhat different idea of evolution from any that has so far been dealt with in these pages.

Fisher has argued that if mutation occurs at random, then the larger the change the less the likelihood that the new organism will be adapted to its environment—indeed that most large mutations are likely to be lethal. Evolution is thought to be brought about by small mutations; and to progress slowly and continuously by the accumulation of factors like those that affect the protein content of maize seeds, or the modifying factors, found in *Drosophila* and elsewhere, that cause small changes in the way a character develops. The importance of these small changes has also

been stressed by developing a familiar argument suggesting that characters like dwarfness in the pea depend not only upon the factor *t*, or whatever it may be, but upon the way in which *t* reacts with all the other factors of the organism. According to this argument, since *t* could clearly do nothing without the other factors, then in a different organism, if it could exist at all, the effect of *t* might well be quite distinct from what it is in the sweet pea. It would be for this reason that when a whole chromosome is added to a zygote, as in trisomics, there would probably be a change in nearly all the characters of the organism. Dwarfness, it is therefore assumed, may be profoundly affected by the whole body of factors other than *t*; and these other factors assume a fundamental importance in determining this, or any other, character. In this way, numerous small changes, which might each pass unnoticed alone, may profoundly affect the organism and be primarily responsible for evolution.

A merit of this hypothesis is that it calls attention to the possible importance of small changes, which otherwise might easily be overlooked. There can be no doubt that, since they are easier to trace, the genetical worker selects for study factors that have the largest and most clear-cut effects, and that there may consequently be a tendency to think too much in terms of these factors. It is important that this possible source of bias should be realized. But it is even more important to be cautious about interpreting evolution in terms of modifying factors, which are precisely the factors about which there is least exact knowledge. Their importance is a matter for further study, rather than for speculation. Further, the argument for the importance of modifying factors rests too much upon the assumption that new types differing widely from the old will not be likely to survive. The occurrence of polyploidy, and other

changes in chromosome number or organization, shows that this assumption is often untrue.

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CHAPTER XI

SELECTION

AT first sight, all that Darwin needed to assume for his theory of natural selection was that a special type of animal or plant could be multiplied by selecting it ; and this assumption seemed to be amply justified by the general experience of plant and animal breeders. To-day, however, exceptions are well known. Thus, a heterozygous form will never breed true, no matter how long it is selected ; it is for this reason that attempts to fix the Blue Andalusian fowl or the Dexter breed of cattle have always failed. Nor could a fluctuation caused by the environment ever be perpetuated, except by the environment itself. More important, however, is the fact that selection cannot continue indefinitely to be effective. To Darwin it seemed that there need be no limit to what selection could do. Now we know that, in the absence of new mutations, selection can only isolate the most extreme genetical type from a population. Once this type has been isolated it will breed true and selection will cease to be effective ; we cannot continue to alter the size, shape, or other attribute, by selecting the extremes.

If the truth of the Mendelian and pure-line principles be admitted these conclusions follow readily enough. Nevertheless, since Mendelian analysis allows us to express exactly how any individual of known composition will breed, it will be useful to find out exactly what the effect of selection

should be in a known case and to see whether this expectation is fulfilled in practice. In the simplest cases the calculation is easy. In others it becomes difficult; but has been taken up successfully by Fisher and Haldane in England, and by Wright in U.S.A.

In the case of a population consisting of two types, A and B, which do not interbreed, it is evident at once that if A reproduces faster than B the proportion of the latter will decrease, and if the population does not grow in total numbers B will become extinct. Even if the advantage of A is small, so long as it is enjoyed in every generation, the replacement of B by A will be fairly rapid. Thus, if the two types are originally present in equal numbers and A is favoured, so that for every 10 offspring it leaves 9 are left by B, then after one generation the proportion of B to A is $\frac{9}{10}$. After 10 generations it is $(\frac{9}{10})^{10}$, or little more than 1 is to 3. If A leaves 3 offspring for 2 left by B, then A will be about 8 times as numerous after 5 generations and over 60 times as numerous after 10. It does not need a very serious disadvantage to produce fairly rapid extinction.

There can be no doubt that extinction has been a potent factor in producing the present distribution of animals and plants; and examples of rapid changes in the numbers of a species are common: even during the last thirty years there has been in England a great increase in the numbers of the starling and a decrease in those of the lapwing. At the same time, the simple calculation given above assumes that B suffers an equal disadvantage every generation. With artificial selection this is easily realized. With natural selection it would be difficult to be sure that in a given case a disadvantage would be permanent, since conditions are never the same, and the advantage of A over B might easily be reversed by a change in the density of the population or

in some other way. When B became rarer it might cease to suffer competition from A. Moreover changes in conditions are often large and sudden. In the contest with climate thrushes and blackbirds suffer severely from a single hard winter, but one or two mild winters quickly restore them to their first abundance.

Usually, however, we shall wish to know the effect of natural selection when the different types interbreed. The simplest case is that of one Mendelian factor, when there are three types—*AA*, *Aa*, and *aa*; sex linkage will not be considered. It can be shown that, whatever the proportions in which these three types were originally present, if they mate together at random equilibrium will be reached at once so long as each type produces the same number of offspring (see Appendix). The three types then continue in the constant proportions

$$u^2\,AA + 2u\,Aa + 1\,aa$$

where u is a number depending upon the proportions in which the three types were originally present.

It can easily be seen, for example, that a population consisting of $3\,AA + 2\,Aa + 1\,aa$, mating at random, would give offspring in the proportions $4\,AA + 4\,Aa + 1\,aa$:

	$3\,AA$	$2\,Aa$	$1\,aa$
	↓	↓	↓
gametes	$3\,A$	$1\,A + 1\,a$	$1\,a$
total gametes	$4\,A + 2\,a$, or $2\,A + 1\,a$		
progeny	$(2\,A + 1\,a)^2$ or $4\,AA + 4\,Aa + 1aa$		

In this progeny population $u = 2$ and the proportions of the three classes of zygote will remain constant, so long as mating continues at random and each type has the same number of descendants.

What we want to know is the effect that natural selection has upon a population that has reached this equilibrium. It has been found that the effect produced depends greatly upon whether a small or a large proportion of dominants was originally present. If there were very few, then even slow selection for the dominant will make it increase rapidly in proportion : roughly, supposing that owing to selection the dominants produce k times as many offspring as the recessives, then the proportion of dominants will be multiplied by k in each generation. When there are many dominants this no longer holds, and even with stringent selection the proportion of recessives only diminishes very slowly.

These principles will be illustrated by some examples of both artificial and natural selection. In all of them, complete dominance will be assumed.

The most stringent selection is practised by breeders of animals, or of cross-fertilized plants. It illustrates very clearly how the results depend on the proportion of dominants present. Supposing the dominant character to be the one wanted, then only dominants would be selected for breeding, so that selection is as stringent as it can be. If the dominant were originally present only in very small proportions, which would be the case for example if it were a recent mutant, then the individuals selected will almost certainly be heterozygotes. Their progeny will be 3 dominants : 1 recessive, and this first selection has increased the proportion of dominants from, say, 0·001 to 0·75. In an effort to fix the type, selection would be continued for many generations, recessives being discarded as soon as they appear ; but the proportion of dominants would only increase slowly since the recessives would be constantly segregated by the heterozygotes. (*See next page.*)

after 1st selection	$1AA + 2Aa + 1aa$	propn. of dominants $\frac{3}{4}$
	↓ ↓ ↓	
gametes produced	$1A \quad 1A + 1a$ (discarded)	
total gametes	$2A + 1a$	
random mating after	$(2A + 1a)^2 =$	
second selection	$4AA + 4Aa + 1aa$	propn. of dominants $\frac{8}{9}$
	↓ ↓ ↓	
gametes produced	$4A \quad 2A + 2a$ (discarded)	
total gametes	$6A + 2a$ (or, $3A + 1a$)	
random mating after	$(3A + 1a)^2 =$	
third selection	$9AA + 6Aa + 1aa$	propn. of dominants $\frac{15}{16}$

In the fourth and following generations the proportion of dominants will be $\frac{24}{25}$, $\frac{35}{36}$, $\frac{48}{49}$, etc. Figure 25 shows how

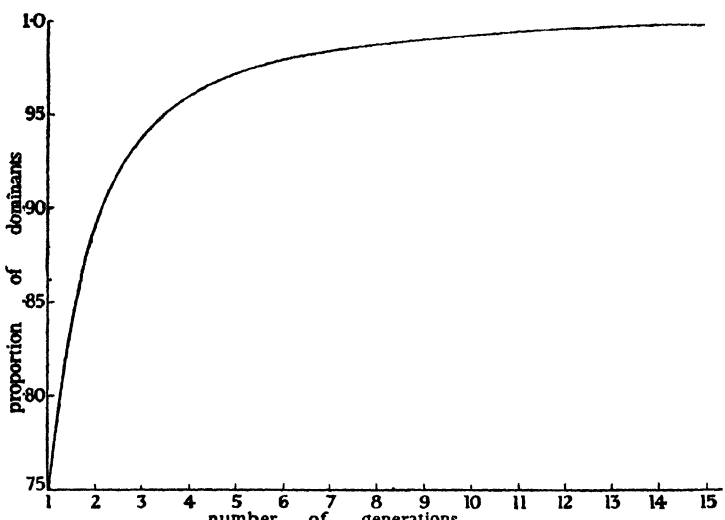

FIG. 25.

the dominants increase in each generation. Even after generations there will still be about 1·5 per cent. of the ur wanted recessive, and after 13 generations about 0·5 per cent. thereafter the recessives diminish almost imperceptibly.

This shows how difficult it is in a random mating population to get rid of recessives, which are continually replenished by the heterozygotes. An illustration is the mentally defective in man. This term covers several kinds of abnormality, not yet properly classified and studied; some, however, are undoubtedly simple recessives and others are probably inherited in the same way. Supposing that all were due to the same recessive factor, and that the proportion of defectives is about 0·5 per cent., then if all defectives were sterilized, so that only dominants were selected, the numbers of recessives would decrease very slowly, being halved in about 6 generations, or from 150 to 200 years. Sterilization would, therefore, not lead to very rapid improvement, but would prevent an otherwise moderately rapid increase of defectives.

A similar example is given by cross-fertilized plants such as maize. Maize plants appear commonly to be heterozygous for one or more recessive defects, which are segregated if self-fertilization is practised; but any one of these recessive factors is present in a small proportion of plants and rarely appears under the usual conditions of cross-pollination. In other words, populations of maize contain a number of rare, disadvantageous, recessive factors.

So far, only very stringent selection has been considered. In nature, it is quite likely that selection will be stringent against a new form and procure its early extinction; but when a newly appearing form is favoured it is not likely to be greatly favoured—selection will be less stringent. Under these circumstances a new recessive has little chance of establishing itself; but, by contrast, a new dominant mutation even if only slightly favoured will spread through a

Assuming that defectives are evenly spread in a random mating population. If they were more frequent in some localities or social groups than others, as is probable, results would be more rapid.

population more rapidly. Thus, if originally present in $(\frac{1}{10})^4$, or 1 in 10,000, of the population, and if it leaves 11 offspring for every 10 left by the recessive, it can be shown to increase to 90 per cent. of the population after about 80 generations.

Probably the spread of certain varieties of moth, in England and other places, is a case of this kind. In several species of moth, black, or dark, varieties have arisen during the last 100 years ; especially, but not exclusively, in industrial areas. In many localities they have largely replaced the original type. These melanics were probably mutants. They are the dominant form and broadly speaking their spread seems to be comparable to the case just worked out—that of a dominant having a small selective advantage. The parallel is not exact since in the hypothetical case the dominant was supposed to be evenly spread, and in the moth, instead of this, the new form would first appear in one place, and besides increasing in numbers there would spread slowly outwards ; so that in any area the number of melanics would depend partly upon their multiplication in that area and partly upon their entry from more thickly populated districts. However, the two cases are not very different and the rate of spread worked out above is comparable to that actually observed. Thus in one species, *Amphidasys betularia*, the wholly black variety *doubledayaria* was first noticed in about 1848–50 in the neighbourhood of Manchester. From there it spread outwards, reaching Norfolk, Essex, and Cambridge about 1892 and London in 1897. In Huddersfield the black form first came later than 1860 but is the only type to-day. At Newport in Monmouthshire the two forms were roughly equal in numbers in about 1870, but a few years later the type was no longer found.

In most of the examples known the melanic form has probably originated in one place and then spread outwards.

In some it may have arisen independently in more than one place : the melanic *A. betularia*, for example, seems to have arisen independently in England and in various industrial regions on the continent of Europe.

It may be noted that the evidence does suggest a connexion of some kind, which has not been explained, between melanism and modern industrialism ; but, at the same time, some melanics have also been recorded first from rural districts.

A case like the melanic moths is that of the birds known as Honey-creepers, *Cœreba*, in the West Indies. Here also a black form has arisen independently in several different places since the middle of the last century and, on some of the islands at least, is replacing the typical form.

With a small selective advantage the spread of a dominant through a population may be slow : in the end it is inevitable so long as the advantage persists. The same is true, indeed, of a recessive ; but in this case the time required is very great. The significance of this for evolution, however, is very uncertain, because there could be no assurance that a very small advantage would be retained through all the vicissitudes, some of them no doubt large, that are likely to occur in the long periods needed for the new type to establish itself. It is more probable, arguing solely on theoretical grounds, that definitely advantageous types might spread rather quickly, while others fluctuated in numbers with changes in the environment.

When a character is due to many factors, especially if mutation may occur and in cases where the total population is not large, mathematical analysis of the effect of selection is difficult, but in some special cases important conclusions have been reached. Thus Haldane has shown that, in general, selection tends to favour not the type with the highest average value, as might have been expected at first sight.

but the type that is most variable. The fact that the most variable type is selected means that competition tends to favour high response to the environment.

Further consideration will only be given here, however, to the so-called quantitative characters, in which inheritance is believed to depend upon the cumulative action of many independent factors. The effect of selection in this case is very important to the practical breeder of animals or plants, if not to the student of evolution; and while the general effect will no doubt be similar to that in the case of the single factor, except that it would be much slower, there are still certain definite questions to which an answer is wanted. Thus: How big an effect will selection have? Will the effect persist when selection no longer operates? How many factors must be postulated to explain the results, and is this number reasonable?

Selection experiments that will give the kind of information the geneticist needs are not easily carried out. Some interesting results have been obtained, however, at the Illinois Agricultural experiment station in America by Winter. Here, selection for high and low percentage of protein and oil in maize seeds has been carried out since 1896; though only the results for the first 28 years, to 1924, have been reported so far. Most remarkable is the magnitude of the change brought about, and the fact that selection has been effective throughout the period.

Only the results for oil content will be described; those for protein were similar.

In 1896 the mean oil content of 163 ears was 4·68 per cent., the highest and lowest values occurring being 6·0 and 3·9 per cent. Selection was both for high oil and for low oil content. It continued to be effective for the whole 28 years and gave little sign of slowing down by the end of the period. By 1924 the mean percentage was 9·86 in the

high line and 1·51 in the low line—a specially striking result in that the new values are far outside the range of variation, from 3·9 to 6·0, found in the original 163 ears. The extreme variant in the original population was only 1·32 per cent. above the mean; yet selection raised the oil content by no less than 5·18 per cent. above this mean—far outside the original range.

No doubt selection produced results for so long largely because it was not stringent. Maize is a cross-pollinated plant, and after the first 9 years special measures had to be taken to ensure cross-pollination and thus prevent loss of vigour from inbreeding. Consequently, when a plant bearing seeds rich in oil is selected, the pollen parents of these seeds might carry factors for low oil content and many of the seeds would give progeny inferior to the mother plant. Furthermore, the character is clearly one that is subject to much fluctuation, so that an ear selected may owe its high oil content as much to environmental as to genetical factors. And finally, the proportion of ears selected was rather high —in the first year 24 out of 163, or nearly 1 in 6.

The important fact is, however, that selection should give a race with a mean so far outside the range of the original population. The most likely explanation of this—as pointed out by the writer who publishes under the pseudonym "Student"—is that the number of factors influencing oil content is large; so that in a random mating population most of the individuals would be heterozygous for many of these factors.

Thus if *A, B, C, D* . . . give high oil, and *a, b, c, d* . . . low oil, then heterozygotes such as *AaBbCcDd* . . ., *AABbCcDd*, and so on would be common; but the homozygotes *AABBCCDD* . . . and *aabbccdd* . . ., which would give the maximum and minimum values, would be very rare. With a large enough number of factors, the extreme

forms finally selected would give an oil content far removed from that of the numerous, heterozygous, intermediate forms. "Student" estimates that, even if there were no change after 1924, the effect already produced is due to at least from 20 to 40 factors, possibly far more, and is not at all likely to be due only to from 5 to 10; and in the absence of any special genetical mechanism this conclusion seems almost unavoidable. The calculation does not claim to be final, but it certainly suggests that the number of factors concerned in the inheritance of quantitative characters is likely to be large; and this possibility must be seriously considered when experiments on the inheritance of these characters are performed.

Alternatively, the results obtained by Winter may mean that one or more mutations for high oil content had arisen during the experiment, and had been selected. Even so, however, the number of factors originally present is not likely to have been so small as from 5 to 10.

Whatever conclusion be reached about the number of factors needed to explain Winter's results it will still be true that selection has brought about a big change in oil content. As "Student" points out, this capacity for change when selection takes place, in cross-fertilized organisms, may be important in evolution since it would help a species to meet changed conditions. He suggests that the species has a store of factors which have slowly accumulated over a long period, and may be of no use at the time; but under changed conditions a new variation, adapted to the change, is selected.

In this way a new form that was apparently a purposeful adaptation to new conditions, but really a segregate from the store of factors already present, would be isolated.

The conclusions drawn from Winter's experiment recall the geneticists' contention that selection can do nothing but

select the best of the existing types. This contention is undoubtedly correct. The power of selection is limited. But this does not alter the fact that selection can evidently bring about large changes and produce new types lying outside the range of the old. The pure-line theory shows that once a homozygous form has been selected no further change is possible. When, however, in a cross-fertilized organism, a character is affected by many factors it may need many generations of selection before a segregate containing all the favourable factors is obtained.

Too much cannot be concluded from one experiment, but it seems probable that this result would sometimes apply to other quantitative characters ; to characters, that is to say, whose degree of development is known to be much influenced by the environment. The results do not apply to qualitative characters. They suggest a way in which an existing species may become adapted ; whether they show how new species may be evolved must be discussed in a later chapter.

In Nature, the possible effects of selection have been most deeply studied in relation to what is called mimicry. In the simplest examples of this phenomenon, a species that would otherwise fall an easy prey is protected from its enemies because it is constantly mistaken, by reason of its similar colouring or shape, for another species which is immune from attack because it is distasteful or is able to defend itself. In other cases a species may resemble a dead leaf. A familiar example, of deception at least, is that people with memories of the wasp will often be cautious in their behaviour towards flies having the familiar alternating black and yellow banded abdomen.

Some cases of resemblance are certainly remarkable and deserve close study, but the exact interpretation of mimicry has long been a subject of controversy. There is no doubt

that some species are safeguarded from attack by their resemblance to others; there has been disagreement about the exact way in which this resemblance came about. Similarities of the kind in question are particularly common in butterflies and moths, though they are well known in other branches of the animal kingdom. Flies with a close superficial likeness to different species of bee are examples (plate II).

The origin of the resemblance will be best understood by considering first a case described by Vavilov, who was studying at the time the variation in allied species of plants. In Darwin's day it was realized that related species, as we might expect, often varied in a similar way; and after long study of the matter, especially in cultivated plants, Vavilov enunciated the Law of Homologous Series in Variation, which states that the varieties of related species, or the species of related genera, are distinguished by the same series of differences. Thus macaroni wheat, *T. durum*, and bread wheat, *T. vulgare*, both have varieties with either white, red or black chaff, with white or red grain, and so on. In barley, *Hordeum*, in wheat, *Triticum*, and in rye, *Secale*, the colour of the grain may be white, red or purple; and in barley and rye, though not in wheat, green grains may also be found. In some cases this relation led Vavilov successfully to predict the discovery of forms that were then unknown.

In one case, the selection of parallel variants in the lentil, *Ervum lens*, and the vetch, *Vicia sativa*, led to a case of mimicry in plants. Vetch is often found as a weed in crops of lentils, and sometimes the seeds of the two were indistinguishable. On collecting varieties from different parts of Russia, Afghanistan and Bokhara, it was found that the seeds of both plants varied greatly in shape, colouring and markings, and that the variation in one was exactly paralleled

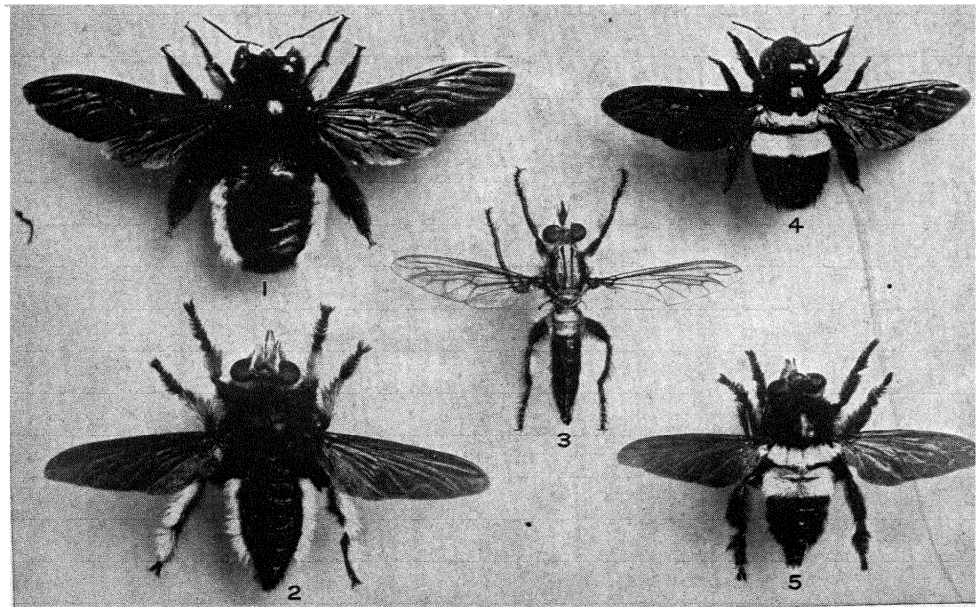

PLATE II. Mimicry (from Carpenter and Ford).

1 and 4, two species of bee, models ; 3, a typical Asilid fly ; 2 and 5, Asilid flies mimicking 1 and 4. In each case, model and mimic obtained from the same tree-trunk.

1. *Xylocopa nigrita*, female. 2. *Hyperechia consimilis*, male. 3. *Andrenosoma vidua*. 4. *Xylocopa inconstans*, female. 5. *Hyperechia bifasciata*, male.

by that in the other. All the facts suggested that, through constant selection by man, only the vetch seeds that could not be separated from those of the lentil were left to be sown, so that the weed might be said to mimic the crop. Similarly, the weed vetch flowered and ripened seed at the same time as the lentil—a further condition for survival that strengthened the likeness between them.

In few cases has variability been studied in so much detail as in cultivated plants, most of which are very variable species. Nevertheless it is likely that the Law of Homologous Series is widely applicable, and it will be recalled that parallel series of coat colours have been found in different species of mammal. It is probably right to regard any species as having potentially a large range of varieties; closely related species having many variants in common, those more distantly related fewer. Mimicry results from the existence of a variant which is protected from enemies by its resemblance to another species. Such a variant would be selected, and spread; and would become the typical form of the species.

Once two parallel variants have been established as the prevailing types, it is clearly possible that resemblance might become closer through the survival of the best mimics. We should therefore expect the occurrence of mimicry to depend upon two independent factors: first, the kind of variations that arise; and second, the kind that is selected. The importance of parallel variation was first pointed out by Punnett. The part played by selection has been the subject of close attention by most students of mimicry.

Mimicry has been most actively studied in butterflies and moths. In this group it is very common, the colour pattern being affected, and the resemblances are sometimes remarkable. Selection is usually believed to depend upon the fact that some species, the models, are definitely dis-

tasteful to birds and monkeys, which soon learn to recognize these by their colour pattern and refuse to eat them. This belief has been confirmed by direct observations. In some cases the unpleasant taste is attributed to a fluid exuded by the insect, and it has been observed that an apparently similar fluid exuded by certain grasshoppers is distasteful to monkeys.

If a distasteful species should have a distinctive pattern this pattern should certainly have a definite survival value; and the best-known models are in fact described as usually having crude colours and a conspicuous pattern. If another species, not itself unpalatable, were to produce a variety with a protected pattern, this variety would probably become the surviving type, being protected from attack, and the species would be described as a mimic. It is therefore found that the closest mimetic resemblances are those between species occupying the same area; though since parallel variants may arise anywhere, we also find forms with a similar colour pattern in unrelated species occurring in different regions of the world.

The typical mimic is strikingly like the model and unlike its nearest relatives. In some cases a variable species will have several different models. Thus, *Papillio dardanus* has several female forms with distinctive colour patterns. Three of these, which have been shown to be simply related to each other genetically, mimic some related species of Danaids; but a fourth form mimics an entirely different species, *Planema poggei*. As we might expect, it is clearly a matter of chance where a variant finds its model.

In the simplest cases the mimicry theory is reasonable. If a species can exist in two varieties, one resembling a protected species, the other not, the mimic is likely to be the surviving type. Complications may arise, however. Thus the polymorphic butterfly *Acræa johnstoni* has in East Africa

several varieties that mimic other species, such as *Danais chrysippus* and various species of *Amauris*. Three hundred miles to the west *A. johnstoni* occurs again ; but the variety found there resembles the species *Planema quadricolor* which also occurs in that area. . In this example both mimic and model are distasteful species and therefore protected. To explain this as the result of natural selection, it has been assumed that it will be an advantage to all distasteful species if they have a common pattern, since the lesson of their distastefulness will then only have to be learned once.

There may also be other complications and doubt may easily arise whether, in a particular case, the resemblance between two species owes anything to mimicry, as usually understood, or whether other factors are responsible. There are various factors to be considered, and despite the many years that have been devoted to the subject there is still much in dispute. The earliest exponents, as ardent disciples of the original doctrine of natural selection, tried to show how remarkable it was that a variant like the model should be found in the mimicking species, and that its existence was a proof of the great power of natural selection. The importance of parallel variation was only pointed out much later ; but to-day many beautiful examples of this phenomenon are well known, not only in Lepidoptera, Diptera, and Hymenoptera, but in the flower colour of *Dahlia*, *Begonia*, and *Antirrhinum*, the coat colour of various mammal species, and innumerable other cases : the phenomenon is one of the most widespread and striking in the plant and animal kingdoms. In cases of likeness between two species the possible interpretations are various, and before a conclusion is reached the following factors should be studied : variation within each species and in allied species ; the geographical distribution of the different types ; the possible adaptation of special types

to special localities; the enemies of the two types; and so on.

The importance both of parallel variation and of natural selection, in bringing mimicry about may be agreed; how much importance to attach to each in any special case is still uncertain. But in any event the existence of parallel variations is an important fact, strongly suggesting that the genetical organization of even distantly related species may have much in common.

APPENDIX

1. Equilibrium of one factor in a random mating population.

It may be supposed that the three types *AA*, *Aa*, and *aa* are present in the proportions

$$v\,AA + 2u\,Aa + 1\,aa$$

We wish to find what values v and u must have for the proportions of the three types to remain unchanged when they mate together at random, supposing that the different types produce equal proportions of offspring.

If each individual produces the same number of gametes, the gametes will be

$v\,A$ from *AA*, $u\,A + u\,a$ from *Aa*, and $1\,a$ from *aa*

The total will be

$$(v + u)\,A + (u + 1)\,a$$

Random mating between these gametes will give the following population

$$(v + u)^2\,AA + 2\,(v + u)(u + 1)\,Aa + (u + 1)^2 aa$$

or

$$\left(\frac{v + u}{u + 1}\right)^2 AA + \frac{2(v + u)}{(u + 1)} Aa + 1\,aa$$

The original population was therefore in equilibrium if

$$\left(\frac{v + u}{u + 1}\right)^2 = v, \text{ and } \frac{v + u}{u + 1} = u$$

Both equations are satisfied if

$$v = u^2$$

and the population

$$u^2AA + 2u\,Aa + 1\,aa$$

will be in equilibrium for all values of u. It will be noticed that this would not apply to sex-linked factors.

2. After mating together at random, the population

$$v\,AA + 2u\,Aa + 1\,aa$$

gave a progeny population of

$$\left(\frac{v+u}{u+1}\right)^2 AA + 2\left(\frac{v+u}{u+1}\right) Aa + 1\,aa,$$

which is of the form $u^2AA + 2u\,Aa + 1\,aa$, and is therefore in equilibrium whatever the values of u and v. It follows that a population consisting of the three types AA, Aa and aa in any proportions will reach equilibrium after a single generation of random mating; provided only that all three types are equally productive.

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CHAPTER XII

SPECIES

IF evolution is constantly bringing about a change from one form of life to another, it might be expected that the process could easily be observed by examining a wide range of the animals or plants found in the World today. It should be possible to arrange these, sometimes at any rate, in a long series showing the steps by which one form has passed into another, widely different, form.

In practice this expectation is certainly not realized. One of the outstanding features of the plant and animal kingdoms, indeed, is that usually, instead of forming continuous series, the different individuals can be classified into separate groups known as species, genera, and so on. We must therefore ask how far this classification is valid; and, knowing what we now do about variation, should we have expected it. Attention will be given chiefly to the classification into species, which is becoming clear as the result of the detailed study, both genetical and systematic, given to special groups like *Triticum* (wheat). About the larger groups less, so far, is known.

Reflection shows that while the idea of classifying living things is of very general application it is also liable to misuse. Its reality is shown by the fact that with the growth of language different names have been given to different groups, and that these groups can be distinguished easily without special knowledge. Terms such as grass,

fish, bird, and so on, are readily applied ; and are applied in a way that corresponds to the scientific grouping into families or larger aggregates. Besides these larger groups it is quite usual to distinguish smaller ones, such as buttercup or wolf, which correspond to a genus or part of a genus. Sometimes a distinction is drawn between different species, such as rook (*Corvus frugilegus*, L.) and jackdaw (*C. monedula*, L.) or primrose (*Primula acaulis*, L.) and cowslip (*P. veris*, L.) ; but this is probably less common, since the species of a genus are not always so distinct as in these examples—it is not evident to everyone that there is more than one " kind " of buttercup.

With the awakening of a serious interest in animals and plants, it was natural that one of the first tasks should be to collect as many different kinds as possible, to describe, and to name them. This work made rapid strides after Linnæus invented the binomial system of nomenclature in the eighteenth century, and it may probably be said to-day that most species of flowering plants in the world, and the great majority of animals, in some of the more important groups, have been named and described.

One conclusion may definitely be drawn from the successful development of systematic botany and zoology : the conception of species—that individuals can be arranged into easily distinguished groups—must be very often valid, or the task of giving names would have been abandoned long ago. On the other hand we may suspect that, since naming has been the object and ability to give a name is accepted by most of us from childhood without question, the species conception is likely to be often misapplied. In fact, the hypothesis that living things occur in well separated groups to which different names may be given is not strictly true. It may often be applied but to try to apply it universally leads to confusion. In some groups the divi-

sion into species is not done as easily as the giver of names could wish, and different authorities give different views. Linnæus classified the primrose and the cowslip as separate species ; Bentham and Hooker called them different races of the same species. Moreover, when collections have first been made from distant countries a single specimen has served as the basis of a new species ; but more extensive later study shows, perhaps, that this specimen gives a very imperfect impression of what is really a rather variable species and when adequate material has been collected the whole genus needs reclassifying.

It is difficult to say exactly how definite species really are. In animals Bateson thought that in the commonest cases species were quite definite, with no intermediates ; but Bateson—to whom, more than anyone, geneticists are indebted for his insistence on the reality of species and his vision of the problems so raised—probably over-simplified the matter.

Robson, at the British Museum of Natural History, who has given much study to the subject, in animals especially, is inclined to believe that Bateson was right so far as insects and vertebrates are concerned, but wrong in the case of invertebrates—molluscs for example. Among flowering plants there is no universal rule. In the genus *Rosa* (roses), hybridization between the different forms has made classification into species—in the usual sense of this term —impossible ; in the grass family, Gramineae, it is sometimes easy and often confusing ; and in the family Umbelliferae the species seem to be well defined. In cases like the last named, however, it would be wrong to assume too lightly that further study will not reveal obscurities : more careful observation, or searching unexplored regions, has often done so in the past.

Finally, it is impossible to say exactly what a species is.

Classification, we have seen, is intimately bound up with giving names to easily recognizable groups. Yet *Drosophila melanogaster* and *D. simulans*, which are called different species, are so alike that they are only distinguishable from one another after careful examination ; and it sometimes happens that forms belonging to the same species are so different in appearance that their relationship was not at first recognized. The justification for this grouping is largely that in the latter case the different forms breed freely with one another, and in the former they give a sterile hybrid. It often happens that species cannot be crossed, and when they can be, give a hybrid that is more or less sterile. Bateson was inclined to adopt this as a definite criterion, but it is clear today that this cannot be done.

It may be concluded that the earlier systematic work, while it has shown that groups often exist, has given the species idea a rigidity it should not have. It was assumed from the first that names could be given ; but the assumption—though it often had success—is frequently misleading and should be made more cautiously today. The true position is, in all probability, that there are several different causes any of which may produce an arrangement of individuals into groups instead of into a continuous series, and that frequently, but not always, the members of one group mate freely with each other but not with other groups. These groups may be large or small, well or ill separated ; and very often the larger contain smaller groups within them. Sometimes, also, they have a different geographical distribution. At present it need only be realized that, although the species concept can be widely applied in the plant and animal kingdoms, to define the limits of an individual species may be easy or very difficult, and within the species itself there may be a wide range of variation or very little. When two species are well defined they usually

differ in many small ways. In the British Isles, for example, the mountain hare (*Lepus timidus*) differs from the brown hare (*L. europæus*) in having a winter coat, shorter ears, whiskers and tail, longer limbs, eight mammæ instead of six, and so on.

The origin of species has been explained in two, exactly opposite, ways. Darwin himself finally thought that they are formed by fission : a single large and variable species becomes separated into two groups, either by a geographical barrier such as a mountain range or from some other cause, and with the passing of time the two groups become more and more different, until they are finally ranked as distinct species, the intermediate forms having died out.

On this view species evolve gradually. They begin by being numerous and wide-ranging. The many now occupying only a small area are relics of a much greater diversity, forms that are dying out, or types long separated from the parent stock.

The alternative view, realized in practice with polyploid plants, is that species originate suddenly by mutation. They begin with one or at most a few individuals, derived from another species by a single step. These few individuals increase, and occupy an ever-widening area : so long as they can hold their own at the start, and having done so can overcome their competitors, penetrate new territory, or find a new way of life. If species originate by fission they need have no special evolutionary importance ; but if they are the result of genetical causes they are very important, and we must try to discover how far such causes really operate.

The second theory—origin by mutation—has in fact been applied by Willis and Vavilov, with considerable success, to explain various features of the geographical distribution and classification of plants.

Willis considers that a species restricted to a very small

area, though it may be dying out as Darwin believed, is just as likely to be a new kind that has not had time to spread. From this premise it is possible to draw some important conclusions ; but it will be most convenient to describe first the results of Vavilov, who with his fellow workers has investigated in detail the geographical distribution of the varieties of nearly all the more important cultivated plants. It was chiefly botanical characteristics, unheeded by the farmer, that were studied ; and there can be no doubt that his conclusions would often apply to wild species.

Vavilov found definite regularities in the distribution of many species. An outstanding example is wheat. The genus, as we have seen, contains a number of species, and these were studied separately. The most important, the hexaploid *T. vulgare*, bread wheat, is found in a multitude of different varieties, in which the chaff may be hairy or smooth, and white, red, grey or black in colour ; the grain red or white ; the leaves very hairy or slightly hairy, and the hairs arranged in different ways. Altogether Vavilov described more than 60 varying characters of this sort, and since the different variations are usually independent of each other the number of possible combinations—rough white, rough red, smooth white, smooth red, and so on—is very large. After collecting from all parts of the world it was found that the greatest number of varieties occurred in Afghanistan, where, indeed, nearly every possible variation was found. Leaving this centre there was a continuous falling off in the variability discovered, so that by the time the outskirts of the Old World distribution were reached, in places like England and Japan, only a few characters showed variation and the number of varieties was small ; in the New World, where wheat is newly introduced, the distribution need not be considered.

Vavilov's explanation of this evident regularity is that *T. vulgare* originated in Afghanistan. Here, he considered, in the country of its origin, it would have had the longest time to produce new forms, and with increasing distance from its home the number of varieties would get less ; partly because fewer forms would have found their way there from the centre of origin, and partly because there would have been less time for new variants to be produced.

In addition to *vulgare*, Vavilov found that two other species, *compactum* and *sphærococcum*, had their centre in Afghanistan or northern India. On the other hand it was found that the tetraploid species had an altogether different distribution. They were all centred chiefly in the Mediterranean region, and their place of origin was probably to be found either in Abyssinia or in Northern Africa.

Vavilov's theory appears to be well founded. It gives a simple explanation for a regularity in distribution that must in some way be explained ; and it agrees with genetical expectation, since it finds different places of origin for tetraploids and hexaploids, and this must almost certainly be right. Nor is it difficult to see that a distribution of the kind found for bread wheat is what would be expected if species originate—as most polyploids must be expected to do—in a limited area and from a few individuals, and if they spread without disturbance.

The distribution of some other cultivated plants, such as cotton and potatoes, was equally simple. In other cases the situation was undoubtedly more complicated ; though regularities of one kind or another made it possible for explanations to be given. The different explanations will not be discussed ; but it is necessary to stress the fact that the example of wheat, as here described, must be looked upon as the simplest possible case with no disturbing features. Even with polyploids a much more complicated situation

is possible. In *Spartina*, the new species *Townsendii* probably originated more or less simultaneously both in the English Channel and in the Bay of Biscay ; and it is clearly possible for two diploid species to meet at several points, to give slightly different polyploid forms at each one, and for the subsequent spread of these new forms to give a rather confused distribution from which deductions would be difficult to make. It is therefore proper to conclude that Vavilov has introduced a general method—that of studying systematically the distribution of variation within a species —which may be applied to any species. In some cases definite information about the origin of the species may be obtained—since a simple type of distribution is often found ; but there are many possibilities, and it might be found that the distribution was not simple enough for any definite conclusions to be drawn.

The views of both Willis and Vavilov are based on similar principles, but Vavilov's methods apply to particular species, so that definite information may be obtained in each specific case. Willis, on the other hand, ignoring the differences between one species and another—though recognizing that they exist—points to certain conclusions likely to follow on the average. About any particular case no conclusion can be reached. He begins by making the assumption, which there is reason to believe is sometimes valid, that species originate as one or a few individuals. Good evidence is given for believing that normally the rate at which plants spread is very slow—though this is not true of the new *Spartina*—and it is argued that the length of time a species has had in which to spread, its age, is a major factor in determining the area it covers.

There are no doubt other factors to be considered. Indeed, the mechanisms by which seeds are dispersed are so various that different species of plant are likely to travel

at very different rates. In the dandelion and other Compositae, for example, the seed may be carried a long distance by the wind, and should it establish a new plant a second crop of seeds is ready for dispersal in a year's time. The spread will then be comparatively rapid. But a tree with heavy seeds is differently placed. Unless there is a special mechanism for dispersal its seeds will only be carried a short way, and when a new plant is established several years will elapse before the next generation is ready.

Willis, however, suggests that these factors can be ruled out to a large extent by comparing two series of species that belong to the same family and do not differ conspicuously in the way that trees and herbs do. When this is done, he considers, it will usually be true that on the average the most widely ranging species will be the oldest and those occupying the smallest areas the youngest. Thus in Ceylon, as in most restricted areas, there are a number of endemic species: species, that is, that occur there and nowhere else. On the older Darwinian view these are relics, species that have succumbed under the pressure of natural selection and are now only to be found in a few favoured spots where conditions specially suit them. Willis, on the other hand, while admitting that some endemics may be relics, thinks that on the average they are most likely to be new species that have not had time to spread further.

He was first led to suppose this because the Darwinian belief had no support in any other observations. Thus, in any particular genus he found that the species might occupy widely differing areas, but he was quite unable to find any characters that would explain these differences in distribution on grounds of adaptation.

Again, the endemic species often occupied only a very small area on the island and therefore, according to the hypothesis, should be adapted to special local conditions

within the island. Yet it was difficult or impossible to find any evidence in support of this. On his own theory the reason that the endemic species so often occupy a small area, even within the island itself, is of course that they are newly arisen species that have not had time to spread far. Those that are not endemic are older species and therefore should, on the average, occupy a larger area in Ceylon itself than the endemics do. Indeed, the more widely a species is spread outside the island, and therefore the older it is likely to be, the more widely it is likely to be spread inside the island itself—since it will probably have been present on the island a long time as well.

Willis also finds that wide-ranging species are the rarest in the world at large; but this result probably has nothing to do with age and is far more likely to mean that few species can tolerate a wide range of conditions. This is supported by Johannsen's figures for the number of animals that can tolerate various ranges of salinity in Rander's Fjord, the distribution being of the kind that Willis finds.

Range of Salinity.	Number of Marine Species restricted to this Range.
20–24 p.m.	16
15–24 ,,	9
12–24 ,,	4
8–24 ,,	0
6–24 ,,	3
5–24 ,,	3
1–24 ,,	1

In principle, the picture of geographical distribution given by Willis, with which Vavilov's researches agree, may sometimes be a true one and should in many cases make it easier to understand the complex facts of geographical distribution.

On the other hand it is open to objections, of which the most important is that it suggests a unifying principle where it is doubtful whether one exists. Sometimes, as the poly-

ploid shows, a species may originate as a few individuals which spread over an ever-widening area; and clearly age is one of the factors that will decide how far such a species spreads. There should accordingly be cases in which Willis's theory holds; but it would be difficult to say at present how widely it is likely to apply. Though species may originate as a few individuals they may also arise by fission as Darwin suggested, and many endemics may be true relics as he also supposed; for there is no doubt that species do die out and must become restricted to very small areas in the process. How often an endemic should be regarded as a relic, how often as a new species, it is at present impossible to say. And for species far distributed in the world at large to be widely spread in a special area like Ceylon would also be expected on the theory of adaptation. The latter theory would rightly suggest that a widely-spread species must be adapted to a wide range of conditions, whether in the island of Ceylon or outside it.

Even when species do originate as a few individuals, however, it is doubtful how far the effect of age may be isolated from other factors. Willis argued that new species spread slowly; but the spread of *Spartina Townsendii* has been rapid.

It may be said that Willis's theory tells us what might happen in some cases. Unlike Vavilov's it does not tell us in which cases it actually has happened, and very often it is this particular knowledge that is needed. At the present time it is probably wrong to suggest any general method for the origin and spread of species; but Willis's theory does give a way in which species may spread sometimes, and it is for the investigator to find those cases in which his theory is applicable. Rightly applied it should be valuable.

A further principle, essentially similar to Age and Area,

has also been suggested by Willis. Extending the idea that new species arise suddenly, he supposes that new genera also may first come by the production at a single step of a few individuals that differ widely from their progenitors. In this way, a new genus would start as a single species. Later, further species may be evolved from the first; and in general, the older a genus is the more species on the average would it contain; monotypic genera, those with only one species, being usually therefore the youngest.

Willis then showed that monotypic genera are much the most frequent, whether in a single large family or in the world as a whole. The next most frequent are those with two species, then those with three, and so on, the largest genera being the least common. Taking for illustration the British flora only, the following results are obtained:

Number of Species in the Genus.	Number of Genera with this Number of Species.
1	223
2	90
3	35
4	32
5	16
6	15
7	5
8	7
9	2
10	6
—	—
—	—
—	—
71	1

Similar results are obtained for the flora of other regions, or of the whole world. The frequency of the large genera naturally becomes rather irregular, but they are clearly much the rarest. When results of this kind are plotted a logarithmic curve is obtained; and Yule has shown mathe-

matically that a curve of this type would actually be obtained on the assumptions made by Willis, that genera and species arise by single steps.

Agreement is close ; but once again the conclusions to be drawn are uncertain, since a similar curve would probably be obtained on the opposite assumptions. On the older Darwinian belief a genus is formed by the dying out of connecting links, giving groups of species isolated from their relatives to a greater or less degree. If this dying out occurred by chance, it is not difficult to see that, although a single species is fairly readily isolated, it is far less likely that a large group would become separated from its neighbours and at the same time remain unbroken. Following this line of argument reasons can be given for believing that the Darwinian assumption would also give the logarithmic type of distribution. The regularity of Willis's curves need explanation ; but, since opposite hypotheses are likely to give this result, no definite conclusions can yet be drawn.

This introduction has revealed two explanations for the origin of species : gradual evolution ; and sudden origin. Much discussion has centred round these opposing views, so far without agreement ; though there are signs that genetical investigations may take us further. The following discussion will therefore show the genetical method of approach ; but no discussion at the present time can be unprejudiced, since definite knowledge is limited to a very few cases, and it is impossible to say how far it is safe to proceed from these.

Moreover, past theories have ignored genetics and sought to explain everything as the result of selection. In pointing out the importance of genetical factors, selection, in its turn, must not be overlooked.

The problem of species is best approached by considering what it is that keeps existing species separate once they have arisen. Very often it is some form of sexual incompatibility,

either failure to produce a hybrid at all or the production of a sterile hybrid, and this method is undoubtedly the most important. There are others however. Two species may occupy different areas and never meet. Animals may differ in their mating habits, or in the season of mating; plants in time of flowering. It is also conceivable that two species, though differing in many characters, remain distinct because of some genetical mechanism; for example because their differences are inherited as a single unit. The tetraploid wheat, *T. polonicum*, is a case in point. This is a striking type with very long papery chaff, like that of oats, and a very long grain. Until recent years systematists never hesitated to classify it as a distinct species, forms intermediate between it and other species being obvious hybrids. When it is crossed with *durum*, however, all the differences are inherited as if they are caused by a single factor, segregation into 1 *durum* : 2 intermediate : 1 *polonicum* being obtained. Even if the genetical difference is really due not to one factor but to a group of completely linked factors, we have a simple, though perhaps not very common, reason for the existence of a well-defined group of individuals to which the term species has been quite naturally applied.

But while all these mechanisms may together be common, they are undoubtedly overshadowed in importance by the factors of hybrid sterility or incompatibility. We need not agree with the view "commonly entertained by naturalists" in Darwin's day "that species, when intercrossed, have been specially endowed with sterility, in order to prevent their confusion" (*The Origin of Species*, p. 219), but we can be sure that a great deal of confusion has been prevented by this means.

Indeed, some form of sexual incompatibility between species is so common that many naturalists have wanted to use this factor, in all doubtful cases, as a criterion of

specific rank. Modern results, however, show unquestionably that it cannot be so used. A clear example of this, out of many, is given by autotetraploids. In *Primula sinensis* the tetraploid is almost identical with the diploid, being easily mistaken for it, and the question of giving it specific rank would never be considered. Yet the two forms cross with the utmost difficulty and the hybrid is almost sterile. In short, such a criterion could only be used to define what cannot be defined and to draw a sharp distinction where none should be drawn.

Nevertheless the subject of hybrid sterility is one of the most important for the species problem. Sterility is not only a common attribute of species hybrids, but is one whose origin, on any theory of natural selection, has always been difficult to account for. Even in this century it has been a reproach to students of evolution that although they could point to the origin of new variations within historic times, and even to the production of widely different forms from a presumably common stock, in none of these cases was there even a hint of the origin of interspecific sterility.

Darwin considered that it occurred after varieties (a lesser distinction than species, supposed to apply only to two mutually fertile forms) " have been permanently modified in a sufficient degree to take rank as species " (*The Origin of Species*, p. 240). But this does not remove the difficulty, as Darwin himself saw. If a new form cannot breed successfully with the types from which it arose, how did it ever establish itself? It might be imagined that two groups of individuals became separated by some geographical barrier, and as the result of the different conditions under which they lived became progressively more unlike. If this change affected all the individuals of a group at once, so that while they did not cease to be fertile among themselves they became sterile with members of the other group, the

matter could be understood. But the suggestion of mass transformation raises great difficulties, and is certainly quite contradictory to all that genetics teaches us about the origin of new variations. Yet if a new type arises by mutation and is sterile with the old, then unless it can be self-fertilized, which can happen only in a limited range of organisms, several individuals must have arisen simultaneously.

At first sight this appears to raise difficulties, but it can happen in two ways. In the first place, the circumstances that give rise to the gametes of new type, the mutant gametes, might cause many to arise at the same time. A second possibility is, in principle, that the new form is recessive to the old, so that a mutant gamete *a* gives first a heterozygote *Aa*, from which back-crossing to *AA* gives more heterozygotes, and from these a number of recessives *aa*, unable for some reason to mate successfully with the dominant, are produced.

The subject is one that has shown considerable genetical difficulties, not only because the incidence of sterility seems often capricious, but also because of the obvious hindrances to analysing hybrids that are partly or wholly sterile. In many cases the difficulties remain, but in the special case of the flowering plants it is probable that the factors responsible for preventing a successful mating between two different forms are now understood, and there is no reason to suppose that this group gives a misleading picture of the general principles. Analysis depends, however, upon showing the exact stage at which the reproductive process fails, and why it fails; and the reasons for failure will differ in groups with different reproductive mechanisms.

The clue to the matter was given by the discovery that species often differed in chromosome number, so that a hybrid between them would have irregular chromosome behaviour. Even if two species had the same number of chromosomes, and probably these cases are the more funda-

mental, the same result would ensue if the two sets differed in ways that prevented them from pairing regularly.

Irregularity during meiosis means that the cells produced by these divisions are of very varying and unusual genetical constitution ; and in wheat hybrids it has been clearly shown that, in consequence of this fact, some of the pollen and the egg-cells, as well as the seeds they produce, may not develop properly. Similar results have been obtained in other genera—pollen tube growth especially has been fully studied in *Datura*—and it may be accepted as a general rule that irregular chromosome behaviour will lead to some degree of sterility. Sometimes sterility may have other causes, and in some respects there may be much still to be learned, but for the present it will not be wrong to relate infertility to genetical or cytological differences that are able to interfere with the normal pairing of the chromosomes.

A second phenomenon, rather different but equally important, is incompatibility—the failure of crossing to occur. This may apply to closely related varieties and, similarly, some plants are self sterile, but its special interest lies in the fact that it is often found to be true of attempts to cross two species. Incompatibility is not a simple matter, essentially because reproduction in flowering plants, as in so many other organisms, is a highly-specialized process ; but though there is still much to be learned about it, the chief factors concerned will be indicated.

In flowering plants, pollen containing the male gametes, with n chromosomes, is conveyed by mechanical means, such as wind or the visits of insects, to the stigma of another plant, where it sends out a pollen tube that grows through the somatic, $2n$, tissue to the ovary. Here it discharges two male gametes, one of which fuses with a $1n$ chromosome egg-cell to give the $2n$ embryo, while the other fuses with two other female nuclei to give the $3n$ endosperm, as it is

called, upon which the young embryo lives during its growth or during the germination of the seed in the soil.

It has been found that the success of these processes may be influenced both by Mendelian factors and by the chromosome numbers of the tissues concerned; with the result that pollen tube growth and therefore fertilization may fail, or a shrivelled seed, that does not germinate, may be produced. Thus in *Nicotiana*—and the same has been found in other plants by other workers—it has been shown by East and Mangelsdorf in U.S.A. that in some crosses a series of multiple allelomorphs, S_1, S_2 and S_3, determine whether the pollen tubes will grow fast enough in the style to effect fertilization; a pollen tube with the factor S_1, for example, will not grow properly in the styles of plants that also contain this factor. In some respects this effect may be a somewhat special case but it shows beyond doubt that factors affecting the growth of pollen tubes in styles exist.

Again Hollingshead, working in California, has found that a Mendelian factor may affect the growth of the hybrid seedling. Thus, crosses of *C. capillaris* with different strains of *C. tectorum* are sometimes successful; but in other cases the seedlings die in the young stages, and it was found that a single Mendelian factor differentiated the one kind of strain from the other.

Turning to differences of another kind, it has been found both in *Primula sinensis* and *Datura stramonium* that the diploid cannot be crossed with its autotetraploid because the $2n$ pollen tubes from the tetraploid, though they grow quite normally in the tetraploids ($4n$) themselves, will not grow in the $2n$ tissue of the diploids. The same probably occurs in other plants, though it does not occur in all, so that fertilization depends not only upon Mendelian factors but also upon the cytological relations between pollen tube

and style. On the other hand, in *Campanula persicifolia* the tetraploid can accomplish fertilization of the diploid, but owing to the unusual numerical relations that then exist between the various tissues concerned in seed development the seeds rarely develop well enough to germinate properly.

Altogether, it is clear that success or failure in crossing two species depends on the interplay of various independent causes. It is easy to see how it happens that, although it may be impossible to cross two nearly related varieties and possible to cross species from different genera, yet it remains true on the whole that the more distant the relationship the less the chance of a successful cross.

This analysis of incompatibility in flowering plants is not complete, but is enough to show the principles involved, and illustrates the importance that changes in chromosome number have as a means of variation. In the first place, the production of a hybrid is only possible when a suitable relationship exists between a variety of genetically independent tissues: between a $1n$ pollen tube and a $2n$ style, between the embryo and endosperm of the seed and the plant that bears them. These relations are affected both by Mendelian differences and by differences in chromosome number, and the latter alone will sometimes prevent a cross from being successful. Even if it does not, the hybrid produced will be more or less sterile.

In the numerous cases in which species with the same number of chromosomes are incompatible, or give a sterile hybrid, knowledge is less definite. In most of them, however, as in the radish × cabbage cross, the sterility of the hybrid is clearly the result of irregularity in chromosome behaviour, itself caused by lack of homology and consequent failure to pair; and from what has been said it will not be surprising if the differences that prevent chromosome from pairing—whatever their exact nature, about which

far too little is known—often cause incompatibility in crossing, though they need not necessarily do so.

It will therefore be understood that both incompatibility and hybrid sterility are especially likely to result, among other reasons, from variation in chromosome number, or in general by the reduplication and reorganization of chromosomes, though they need not always do so. This is a strong reason why variants of this kind will often, but not always, be classed as different species ; and why species will often show incompatibility or sterility with one another, though sometimes they do not.

It is now possible to answer some of the questions about the origin of species raised earlier in this chapter. In the first place, changes in chromosome number are sometimes examples of species mutations—the origin of new species by single steps. We have seen that new allopolyploids often differ enough from their parents to take rank as new species, and will often fail to cross with them. Such a new species, even if it is not self-fertile and it does not cross with existing forms, is able to establish itself because many new individuals may be produced at once. A single sterile hybrid produces many diploid, " mutant," gametes, from which several identical individuals arise. If, however, the original parents *A* and *B* overlap in their distribution there may be many hybrids, from which a whole population of the new species could come and self sterility would be no bar to its establishing itself. Similarly, a species mutation of any kind is possible if it comes as the result of some factor, such as an irregularity at meiosis, that is likely to produce many of the new gametes simultaneously.

In series other than polyploids, the origin of the different members is imperfectly understood, though evidence was given that chromosome reduplication, and reorganization, sometimes play a part. But whatever their origin changes

in chromosome number are examples of large changes that must have arisen suddenly, and many of them are probably further examples of species mutations.

Further, the true nature of species and other systematic problems are made clearer now that we know the existence of differences in chromosome number and organization ; for these often represent changes that, while large and definite enough to suggest that classification into species will be possible, are so variable in magnitude that the ideal is often difficult to realize. There may be, for example, a complete range from autopolyploid to allopolyploid. The former if recognized at all would probably be classed as a variety, and the latter as a distinct species ; but clearly no hard and fast line could be drawn. Among poppies, various forms of *Papaver nudicaule* have been found to have $2n = 14$; but a form with $2n = 70$ has been classed as *var. striatocarpum* of the same species. In the grass *Festuca* there is a series of forms, some of them polyploids, which are often grouped together as *F. ovina*, L., but the exact limits of the species are a matter of great uncertainty.

In the same way the exact status of the polyploid wheats, whether species, sub-species or varieties, has given rise to constant differences of opinion.

Irregular series reveal a not very different state of affairs. Very commonly the difference between one member of the series and another is greater than that between polyploids, but there is great variation in this respect. In some cases different numbers may be found within the same "species." More commonly, they are found in different species. In other cases different basic numbers are found in related genera, within which there may be polyploid series.

These two kinds of series are only special cases. Very often different species have the same number of

chromosomes, and little is known about the kinds of change that may then be concerned. They may be of a different and more fundamental nature than those so far considered. But these changes in number of chromosomes have added greatly to our understanding of the species problem, and have shown clearly that it is not one problem but many. We know now that an important reason for the existence of systematic groupings is that variations are genetically of different kinds, and that these differ greatly in the magnitude of their effects. The effect of a Mendelian factor is usually small, and often seems to be confined to a single character such as flower colour. It will be regarded as having no special systematic importance—unless, as in *T. polonicum*, a factor has large and manifold effects. Other variations, of which changes in chromosome number ar one example, cause differences of quite distinct and apparently more significant kinds. They suggest the propriety of classifying individuals into groups called species; and the classification has, as might be expected, success enough to provoke further efforts, but not enough to prevent frequent controversy.

The problems of systematics, and their genetical basis, may also be expected to differ somewhat in different groups. For they are intimately connected with problems of sterility and incompatibility, and these may have special features in different groups according to peculiarities in the reproductive mechanism. The genus *Rosa*, roses, is peculiar in having a special mechanism that enables hybrids to perpetuate themselves; even, sometimes, those with irregular chromosome behaviour. Because of this, many of the *Rosa* forms found in nature to-day are unquestionably hybrids, and caused great confusion in the systematics until the cytological investigations of Täckholm, Hurst, and Blackburn and Harrison, made the situation clear.

This account shows that species may arise suddenly; and that many of the phenomena associated with differentiation into species are the result of the kind of variation that has occurred, and can be explained in terms of known genetical changes. It does not show that this is the only way in which species may arise. It is the only way of which we have certain knowledge, but most biologists would regard the formation of species by gradual separation as a probable, and frequent, occurrence.

So far, discussion has been confined to the flowering plants, since far more is known about this group than about any other. Other groups, such as the Animal Kingdom generally, may be similar in many respects but they undoubtedly show also considerable differences. Polyploidy for example, so common in flowering plants, is almost unknown in animals. At the same time, a change in chromosome number or organization is likely to raise the barrier of sterility in any group; so that it is not surprising to find in animals, also, that species differentiation is often associated with such a change.

Because of the importance of infertility, the species problem in any group is likely to depend partly upon the reproductive mechanism, and therefore to differ in different groups. In mammals, and many other animals, compatibility is chiefly a psychological question; repugnance being seen most clearly in animals with highly differentiated mating habits, and probably reaching its maximum in mammals and birds. There is probably a strong tendency for like to mate with like. Statistics show, for example, that in certain physical attributes such as height there is a definite correlation between the partners in human marriages. The domestic dog is a very variable species that evidently has a characteristic scent; and the species is probably defined, in part at least, by the amount of

variation, undoubtedly large, that can occur without causing so great a change in scent that mating will not occur.

In addition to psychology there are other mechanisms that will prevent a successful union. Crosses have often been made successfully between different species of wild and domestic cattle, belonging to the genus *Bos*, but they are unsuccessful when the Indian buffalo, *Bos bubalus*, is the male parent because the hybrid is too large to be born alive. Turning to a quite different group, *Drosophila*, in which most of the species will not cross with one another, mating occurs readily between *melanogaster* and the closely similar *simulans*, but the hybrid is completely sterile because the reproductive organs do not develop properly.

These examples show some of the ways in which there may be a reproductive bar between two species of animal. Fundamentally the issues raised are not different from those discussed for the flowering plants, but outside this group there is no definite genetical knowledge about the matter. It may be concluded, however, that although the gradual origin of species is quite possible, and might conceivably be more frequent in animals than in plants, there need be no objection raised to the origin of species by mutation, which would be quite possible in the conditions discussed for flowering plants.

When specific differences are associated with a change in chromosome number a species mutation is almost certain to have occurred. The change may have come by stages ; first, perhaps, the reduplication of a chromosome, then further changes in cytological organization or factor changes, and so on ; but even on this assumption the original change in number was a large change, comparable to a species change, enough to produce the barrier of sterility and, it may be, incompatibility as well.

It must be emphasized, however, that even in flowering plants very little is known about the genetical nature of the differences between species having the same chromosome number. These may well be of a more fundamental character than those we have described; but cases where the number of chromosomes is different have attracted most attention so far, and have certainly taught us much.

The usual convention, however, has been to assume that species have arisen gradually through natural selection. The process has provided a rich field for speculation, but is difficult to study experimentally. Attempts have been made to get concrete evidence through the study of subjects such as mimicry or geographical races. It was argued that mimicry showed clearly how selection was able gradually to modify one species until it resembled a quite different one; though actually there seems to be no evidence that the evolution of the new species was gradual, and there are in addition two serious objections to this view of the matter.

In the first place, we have seen that mimicry depends at least as much, probably, upon the nature of the variations that arise as it does upon natural selection. More important still, however, is the further fact that mimicry is an example of the selection of varieties within a species and need not have anything to do with the origin of new species. It occurs as the result of the selection of Mendelian mutants; and is in no way responsible for the origin of the other kinds of variation, especially those of a cytological character, which are probably most important in the creation of species. Thus, the forms of *Acræa johnstoni* mentioned on page 178 are classified as varieties of the same species and not as different species, despite their very different appearance.

Another line of evidence is given by geographical races.

When species, of animals especially, are studied carefully it is often found that they differ slightly but unmistakably in different parts of their range. Conceivably these differences are sometimes mere fluctuations caused directly by the environment, but very often they are not. Thus the Common Bank Mouse, *Apodemus sylvaticus*, is widespread on the mainland and islands of Scotland, including Shetland; but on the island of Bute it exists as a distinct variety, differing in numerous small respects, not fluctuations, from the common form. Similarly, in recent years it has been found that several species of British birds exist here in forms having small but definite differences in plumage from the representatives of the species on the continent of Europe. In some cases the difference is greater than in others: the British Red Grouse, for example, has long been recognized as a distinct species from the continental form.

It is suggested that cases like these arise through isolation, which has allowed selection to bring about small changes that would not otherwise have been able to exist. The characteristic fauna of long isolated islands, such as Madagascar and New Zealand, would be extreme examples of the effect of isolation.

Sometimes the differences between races can be correlated with climatic changes. In western America, for example, there are various geographical races of the deer mouse, *Peromyscus*, which are distinguished from each other amongst other respects, by the depth of colour of their coat. Sumner has shown that these differences, which are genetical, are distributed according to variations in humidity, the driest regions being inhabited by animals with the lightest coat colour, and so on.

It has been argued that geographical races are species in the making. Once two groups are separated, variation and selection are able gradually to make them different;

they first become local races, then diverge still further until they are recognized as distinct species. It must be admitted that this process may take place. On the other hand, Goldschmidt, who has studied the local races of *Lymantria dispar* genetically, concludes that they merely lead to diversity within the species, and are not a first step towards species formation. He thinks that local races are means by which the species becomes adapted to varying conditions in the different parts of its range; and that they arise, by the migration of mutations into areas that suit them. But he does not consider that, in their new area, they are likely, any more than any other form, to become further modified and then to be classed as new species.

Discussion of the species problem shows clearly how genetics has contributed towards a better understanding of evolutionary problems. Previously evolution was thought of in terms only of natural selection. It is now clear that it depends just as much upon the nature of the variations that arise. These variations are of different kinds genetically, factorial changes and changes in number or organization of the chromosomes, and this is certainly one reason why the different forms of life can be grouped into species. The idea of species is valid in so far as it emphasizes that differences exist which are unlike those commonly found in an interbreeding population, and often larger. It is artificial to the extent that it includes in one category distinctions of totally different natures and origins, and presupposes a sharp line, where none can be drawn, between the large and the small.

When the only genetical differences known were those due to Mendelian factors, there was no evident reason why there should be species at all; now that other kinds of difference have been discovered this is no longer so. Species exist chiefly because the causes responsible for the differ-

ences between one individual and another are of various kinds, with effects of varying size, so that there seem to be groups with gaps between them ; the distinction between these groups being accentuated because certain kinds of difference are specially likely to prevent successful mating. Sometimes the larger genetical differences arise by a single step ; in other words, species mutations occur. On the other hand, it is widely believed that specific distinctions arise from the accumulations of smaller differences by some form of selection. This is possible, though it is yet to be proved ; but there can be no doubt that two independent factors, variation and selection, are involved in the origin of species. Further than this we cannot generalize. The fact that there are various kinds of genetical difference leads us to expect species ; but we do not usually know in particular cases how much weight to give to the various factors concerned ; the nature of the genetical differences that arose, the reproductive mechanism in the group concerned, the extinction of related forms, and so on.

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CHAPTER XIII

CONCLUSION

IT is now clear that so far as the fundamental principles are concerned the problem of heredity has been solved. The development of any character of course, whether the height of a man or the colour of a flower, is determined partly by hereditary factors and partly by the environment: height is likely to be increased by proper diet and exercise, but under the same conditions some individuals grow taller than others. These hereditary differences are due to constant units, transmitted unchanged from one generation to the next. In the simplest cases the difference between two individuals is due to a single hereditary factor. In others, many factors are responsible.

When development can be measured exactly, as in the case of seed-weight, it will usually be found to be greatly influenced by environmental causes, and at the same time to depend on many hereditary factors. Genetical study then becomes difficult, as it must when the effect of a hereditary factor is small compared with that of the environment, but some analysis can be carried out in self-fertilized plants, where it is possible to isolate different pure lines, containing different factors and breeding true to different mean values. This occurs because self-fertilization gives homozygous individuals, whose progeny all contain the same hereditary factors as themselves. The effect of the environment on development may then be easily seen by growing

these genetically identical individuals under different conditions.

Analysis cannot be carried out so easily in cross-fertilized organisms, because in these it is difficult to obtain pure lines. But in organisms that can be propagated vegetatively the effect of the environment can be seen by growing different parts of the same individual under different conditions; and other experiments that have been made confirm the view that the same principles apply in these as in self-fertilized plants.

Most organisms are built up of cells, which may vary greatly in appearance in the same individual, and thereby give rise to different organs. The hereditary factors present in a cell are organized into chromosomes, usually constant in number for the individual and for the species. When cells divide, the chromosomes divide longitudinally; and by this division ensure that the daughter cells contain the same genetical factors, so that all the somatic cells of an individual are genetically the same.

At meiosis, the divisions preceding gamete formation, homologous particles of chromosomes pair with one another. In a normal diploid organism this produces a constant number of bivalent chromosomes; and the segregation that follows gives cells containing one chromosome, and therefore one hereditary factor, of each kind instead of two. Corresponding, that is, allelomorphic, factors are distributed to the germ-cells in equal numbers; and factors in different chromosomes segregate independently of one another, so that new combinations of factors may be found in the germ-cells. Factors in the same chromosome tend to remain together, but may become separated owing to crossing-over, or interchange of genetical material between pairing chromosomes. The factors are arranged in the chromosome in linear order, and the chance of

separation, or crossing-over, increases with their distance apart.

Normally the organization of factors into chromosomes, or linkage groups, is constant; and usually the number of groups is constant for a species. Sometimes, however, in different individuals of the same species, the factors are differently arranged into groups.

When there are present at meiosis any number of homologous particles or chromosomes other than two, there may be association of chromosomes into threes or fours, or chromosomes may remain unpaired, and an irregular meiosis results. The same will occur when the different sets of homologous particles are not arranged into the same chromosomes; for the particles of one chromosome may then pair with particles in two or more separate chromosomes instead of only one. With irregular meiosis there may no longer be a distribution of allelomorphic factors to the germ-cells in equal numbers. The simple 1 : 1 ratio may not be found, and instead of only one allelomorph there may be 0, 1, 2, . . . allelomorphs in the same cell.

With only two homologous particles of each kind, and regular meiosis giving two kinds of gamete in equal numbers, random mating between these gametes gives the simple ratios, like 1 : 2 : 1, of classical Mendelian inheritance. The ratio is disturbed when a hereditary factor prevents random mating—as with the factors affecting growth of pollen tubes in flowering plants—or causes the death of a zygote that carries it.

When meiosis is irregular, either because there were not two homologous particles of each kind, or because homologous particles from the two parents were not organized into the same groups, inheritance is much more complicated. The hereditary units themselves remain constant, as always, but they are not distributed to the gametes in

simple ratios like 1 : 1. Furthermore, some of the gametes will be sterile, and the zygotes that are formed may not all develop normally. Broadly speaking, the reason for these results is that, after irregular meiosis, some of the genetic elements will be completely lacking in the gametes or zygotes, or they will not all be present an equal number of times, and this has a profound effect on development.

Three kinds of genetical variation have now been distinguished.

The first is variation in genetical factors, the replacement of one factor by a different factor. This has been observed to occur by sudden change, mutation. Changes of this sort usually seem to affect chiefly a single character; and on crossing, the difference is likely to give simple segregation.

The second and third are variation in the way the factors are arranged into groups, and variation in the number of factors of each kind. Together they give great scope for variation. In the production of polyploid series, which in flowering plants often follows hybridization, the origin of an alteration in the number of times the different factors are present is well understood. In most other cases the existence of a rearrangement and reduplication of factors, among naturally occurring forms of life, may be shown from a cytological or genetical study of hybrids; but little is known about their origin. These variations differ from the preceding by commonly giving rise to incompatibility, sterility and irregular inheritance in hybrids. From the evidence of hybrids and aberrant forms, this reduplication of some, but not all, factors is likely to affect a number of characters, some more, some less. The effect on development may be large, and non-viability in gametes or zygotes often follows, as we have seen, but nothing is known about

the conditions determining whether normal development can occur.

The bearing of these results on the theory of evolution may be made clear by considering once more not only Darwin's views but the later theories, such as the Mutation Theory of de Vries.

Darwin's theory rested on three propositions. First, that the individual members of a species are not all alike, as anyone may see. Secondly, that on the average offspring tend to resemble their parents. Thirdly, the existence of a struggle for existence. From this he argued that the best adapted individuals would survive and leave most progeny; and that this selection would lead to continued change.

These three principles of Darwin's analysis—variation, inheritance and selection—must form the basis of any discussion of evolution. But since his day much more has been learned about them, and in some ways they have changed in meaning. Thus, Darwin did not mean by variability what we should mean to-day. He included not only the differences between individuals of a species, but the differences between parents and offspring, or between the offspring themselves. He recognized the existence of sports, or mutations as they are now called, but did not give them special prominence; he thought they would form material for selection just as any other kind of variation would. He believed especially, however, in the importance of changed conditions in producing new variations. When a species was first domesticated the new conditions of life would, he thought, produce many new variants. It may in fact be noted that in *Primula sinensis*, *P. obconica*, and the budgerigar, which have all been domesticated in recent times, a number of mutants have appeared since their domestication, though the reason for this is not known.

Today, we know that not all differences are inherited,

as Darwin thought they were ; and we distinguish between those caused by the environment, fluctuations, and the actual hereditary differences. We understand that change could not go on indefinitely in the way that Darwin believed, and that an important part of the evolutionary problem is the origin of new variations.

With the knowledge available to him, Darwin could not have carried the analysis of variation further than he did, without experiment. He himself saw the need for further analysis, but unfortunately his followers did not. Their preoccupation was with natural selection and adaptation. So far as they considered variation at all, they thought of it as continuous. In other words, they were chiefly struck with that part of it we now call fluctuation.

Although valuable results came from this concentration on one aspect of the subject, it made the theory of evolution a matter chiefly for speculation rather than for experiment ; speculation was sometimes fantastic, and the picture of the evolutionary process distorted. After Darwin himself, no great advance was made until 1894, when Bateson suggested that evolution was not continuous but discontinuous.

Objections had already been made to Darwin's theory ; and some of these were cogently argued by Bateson, who analysed acutely the whole Darwinian position, and pointed out that to understand evolution further the facts of variation must be studied.

He was led to conclude that evolution would prove to be discontinuous largely because of the existence of species, that is to say, because individuals occur in groups which are often well marked and may be distinguished without much difficulty one from another. He found it hard to see how the distinction between species could be sharp and definite if new species came by the slow accumulation of almost imperceptible variations, as was then believed. If the conven-

tional theory were right—if variations were continuous and new forms came as better adaptations to the environment —then these well-marked differences between species could only be explained, he argued, as the result of sharp differentes in the environment: a sudden change in the character of the climate on land, or in the salinity of sea water. But this explanation was ruled out. For, in the first place, abrupt changes in the environment are unusual: as a rule the change is gradual. Secondly, it often happens that related species occupy the same environment. For his part, he sought to explain discontinuity between species by discontinuity in variation, but he stressed the need for settling the matter by observation and experiment. As a beginning, he himself collected evidence of discontinuity in variation, and of the sudden appearance of new characters or organs.

Time has proved him right. He did not make it clear whether he himself thought that species actually arose by mutation at a single step, or by a succession of several changes. But he was right in thinking that variation was discontinuous; and in pointing out that the existence of species must be explained by studying variation. For we now know that species are in large part a reflection of the different kinds of variation that may occur, and that sometimes, indeed, they originate through a single large genetical change.

Although the importance of Bateson's work was not appreciated at the time, it helped to clear the way for the general realization that evolution is a discontinuous process. This came after 1900, when de Vries published *Die Mutationstheorie*. De Vries was the first who clearly distinguished fluctuation, or non-heritable variability. He drew his evidence from the plant kingdom, and definitely put forward the idea that the organism was made up of distinct and

independent units; and that while there might be transitions in the external forms of life, there were no transitions between the units themselves.

De Vries was led to these conclusions in two ways.

First, by a critical study of selection, having special regard to the experience of the breeders of new horticultural, or agricultural, varieties. He argued—as genetics has since shown—that the effect of selection was limited, and was in fact no more than the isolation of types already present. The breeder of new forms worked in one of three ways. He either picked out the best existing type, after which no more could be done. Or he deliberately created variability by hybridization, and then chose the best form so created. Or, occasionally, he selected new mutant forms. Like Bateson, therefore, de Vries looked upon the origin of variations as the central problem.

Secondly, he tried by observation to prove definitely that a new species could arise by mutation, at a single step, from existing species. For this he grew cultures of the evening primrose, *Œnothera Lamarckiana*, for a number of years, and did in fact obtain during this period a number of new types, which he thought might reasonably be regarded as new species.

The thesis that new forms of life arose directly from the old by mutation, in steps, won wide acceptance; partly through the rediscovery of Mendel's results at about this time. But there was some hesitation over accepting the possibility that there could be jumps as big as from one species to another and within the last few years, thanks to the able genetical researches of Renner, at Jena in Germany, and the cytological work of Cleland in U.S.A., Darlington in England, and Håkansson in Sweden, it has been found that most of the new forms arising from *Œ. Lamarckiana* are not mutations at all. The species has been

shown to be genetically a hybrid, with an unusual heredity mechanism. This mechanism, though enabling it to breed approximately true, functions imperfectly, with the result that segregates, apparently mutants, are regularly produced in small proportions, as a consequence of definite irregularities in hereditary transmission.

Mendel's paper describing his principles of heredity was rediscovered by Correns, Tschermak, and de Vries, simultaneously and independently, at about the same time as the publication of *Die Mutationstheorie*. Its importance was immediately grasped, and the consequent growth of the science of genetics threw still more light on the subject of evolution.

In various ways knowledge was more definite. In the first place, since it was shown that heredity consists in the transmission of constant units, there was confirmation for the theory of Bateson and de Vries that variation was discontinuous, and that new forms arise suddenly, by jumps. Secondly, it was finally proved that selection alone could not continue indefinitely to give improvement, as Darwin and his followers had thought it could. Natural selection may select but cannot create. Thirdly, Lamarckian doctrine, at least in its usual form, was finally disproved. For the demonstration of constant hereditary units could not easily be made to tally with the idea that what was transmitted depended in any way upon the development attained by an individual during its lifetime. Modifications by the environment were seen to be mere fluctuations that were not inherited; and all that the environment could do would be to influence the mechanism by which mutations are produced. Nor, so far, is there any evidence that mutations are in any way purposeful, or adaptive.

No further advance in evolutionary theory was made by genetics for some years. Attention had been directed to evolution by mutation, but difficulties soon arose. Not only

did observed mutations all seem to be retrogressive, from dominant to recessive, and therefore not easily able to explain evolution; it also seemed impossible to account for specific differences, which are usually so much greater than the differences caused by Mendelian factors. And so far nothing had been discovered that would make clearer either incompatibility between species or the sterility of their hybrids.

Researches carried out with the aid of cytology, from about 1920 onwards, are rapidly removing the second of these difficulties. It is becoming increasingly clear that, very often, the differences found by the systematist are just what would be expected to arise from the fact that there are different kinds of genetical variation; and similarly that special effects, like sterility and incompatibility, will often follow changes in the numbers of different genetical factors or in the way in which the factors are organized into groups. It is not known, however, exactly how much may be explained by these known genetical causes, and how much may have other causes, yet undiscovered.

Genetics, therefore, has shown that to understand evolution we must understand the different kinds of variation that may arise. Reverting to Darwin's analysis, it is clear that the whole problem of the mechanism of evolution is ultimately the problem of the difference between one generation and the next. This difference depends upon three factors: the way in which characters are transmitted; the possibility that new forms may appear spontaneously; and the selection of one kind of individual for parents rather than another kind.

One result of genetical research has been to dismiss the first of these three factors. The units concerned being constant, the hereditary mechanism in its simple form does not allow change. This refutes the view of Lamarck, which

would be that evolution is the result of inheritance, being due to the inheritance of acquired characters.

There remain two factors to be considered the origin of new forms; and selection.

It is a remarkable fact that, in the past, evolution has usually been considered as due to only one of these two factors; instead of being the product, as it clearly is, of both. Controversy between the two schools has been frequent, reminding us of the old dispute whether the embryo developed from the egg-cell or from the sperm. After Darwin, natural selection came to be looked upon as the sole agent for evolution; and the importance of the origin of variations was overlooked. By contrast, since the time of de Vries the part played by natural selection has faded into the background; though within the last few years there has sometimes been a tendency to return to the Darwinian position.

This last-mentioned development has come from the use of mathematical treatment in discussing evolution. There can be no question that a definite advance is likely to come through the help given by mathematics in working out the effects of selection on populations of known genetical composition. But a general mathematical account of evolution is not yet possible; for evolution depends upon the origin of new variations whose occurrence, so far, is little understood, and cannot be predicted. This difficulty has been avoided by making the assumption, which is not likely to prove generally valid, that new variations occur by chance in any direction. When this is assumed, it will be in the long run only the best adapted variants that survive, so that only adaptation need ultimately be considered. We are forced back to the extreme Darwinian position, and attempts are made to explain by selection effects that really have a genetical basis.

It is now clear, however, that evolution is the product of both factors—the production of new forms, and selection—though we do not know in a particular case how much importance to attach to each. And any theory that rests on only one of them is likely to give a distorted picture of the truth. The importance of selection in leading to adaptation needs no debate : any organism that is not adapted will not live. On the other hand, another characteristic feature of living organisms, the existence of species, certainly comes in many cases from the occurrence of special kinds of variation and is not due primarily to the working of selection.

Genetically, two different kinds of new variation can be distinguished. In one of these, factor mutation, new units are produced from the old. In the other, new units are not produced; instead, the existing units, because of the chromosome mechanism, are rearranged or reduplicated to give the so-called cytological changes. It is largely because of the existence of these two different kinds of variation that the systematist tries to classify organisms into groups, greater importance being attached to the second kind because they may give rise to the barrier of incompatibility or sterility. There is still much to be learned about this kind of variation ; but so far there is no indication that it will not yield to present methods of research.

Many factor mutations seem to be losses, though there seems no doubt that all are not. It could not be stated with certainty, however, that any of the mutations so far observed are likely to have evolutionary importance. This conclusion is unfortunate ; for it is usually assumed by genetical workers that evolution rests upon the occurrence of factor mutation. Furthermore, observed mutations appear to be from one factor to an allelomorphic factor ; that is, to one sufficiently like the old not to interfere with the normal particle by particle pairing at meiosis. A form for

example with the factors *ABCDEF* . . . has been known to give a new form *ABCDEf* . . . Yet it is usually supposed that factors from well-separated species may differ in a more fundamental way; one containing *ABCDEF* . . ., and another *ABCDEG* . . ., for example.

There are several possible solutions to this apparent dilemma. Three of them may be mentioned.

The first has already been alluded to. It is simply that in the short time over which observations have been made there is nothing surprising in the fact that mutations of evolutionary importance have not been observed in the organisms studied.

The second is that evolution is dependent chiefly on small mutations, not on the larger mutations which are seized upon by the genetical worker as favourable for study. The objection to this view is that the small mutations in question seem to have quantitative, not qualitative, effects; they influence, for example, amount of protein rather than kind of protein. For qualitative characters the difficulty remains. Furthermore, great caution must be used before founding a theory of evolution on precisely those factor which are, for technical reasons, most difficult to study experimentally. It may be pointed out, however, that these small mutations—assuming that they do commonly occur—would have a definite importance in one respect. It has been shown experimentally, in maize, that factors with quantitative effects are probably very numerous; so that selection has a large effect and continues to be effective for a long time. This makes it possible for the organism to become adapted to changed conditions. For, as "Student" has expressed it, under stable conditions the organism may accumulate a store of useless factors, which enables it to meet changed conditions when the need arises.

Thirdly, it might be wrong to assume that evolution is

so dependent on factor mutations as is commonly supposed. It is usually thought that for evolution to be progressive, as it seems to have been, new factors must be produced; but there is no real reason for thinking this. A limited number of chemical elements can build up a great number of substances; indeed the four elements carbon, hydrogen, oxygen and nitrogen, alone, are able to give an enormous number of organic compounds of ever-increasing complexity. In the same way it may be that the actual production of new genetical factors is a rare event, especially in higher organisms; and that a limited number of factors can build up more and more new forms of life by reduplication and rearrangement, diversification being assisted by factor mutation of the familiar kind. So far as we know, it is quite possible that some of the fundamental units in very different organisms are the same. It may be significant that chlorophyl, the characteristic pigment of green plants, is chemically similar to hæmoglobin, the characteristic constituent of the blood of higher animals and at first sight a very different substance. There is, in fact, no doubt that factor reduplication has been important; though, except in the case of polyploids, examples of it have not yet been analysed with accuracy. It is known, however, that the reduplication of some only of the factors in a set frequently gives rise to abnormal development and infertility; and until more is known about the conditions under which this will not happen nothing final can be stated about the importance of factor reduplication in evolution.

These three possibilities show that it need not be a matter for surprise that progressive mutations of evolutionary importance have not certainly been seen to arise. But so far nothing definite is known. The answer waits further investigation.

If we return once more to the giraffe, which so usefully

illustrates the various evolutionary theories, it will be seen that there are some conclusions which can definitely be reached, in the opinion of genetical workers, and other matters about which little can be said.

In the first place, the long neck is due to definite hereditary units which are unaffected, so far as we can tell, by any efforts the animal may make in reaching upwards, or by any other environmental factor that affects neck development. Lamarck's hypothesis may be dismissed.

Secondly, Darwin was wrong in thinking that only selection was needed to produce the giraffe from a short-necked ancestor. Selection for quantity is sometimes able to give individuals that lie outside the existing range of variation, and it might have given an animal with a longer neck than any previously known. But, in general, evolution cannot proceed indefinitely by this method; and the production of the giraffe would have to wait upon the origin of the right mutations. In this, de Vries was right.

We do not as a rule yet know, in any special case, how much importance to give to selection and how much to mutation. Among the ancestors of the giraffe, was the advantage of a longer neck so great that any mutation in this direction, however small, was seized upon; while mutations in other directions occurred but did not survive? Or was competition a less important factor, and did the short-necked ancestor have, instead, a tendency to give long-necked mutants, which multiplied because a food supply was available and produced the giraffe by successive stages? No certain answer can be given to these questions.

Again, how gradually was the giraffe produced? Was it slowly formed by the accumulation of small mutations; each successive stage conquering the form from which it came, so that intermediates did not survive? Or did it come by one or a few large changes?

In the special case of the giraffe, as in many others, this question cannot be definitely answered ; though it is known that changes are sometimes large. Sometimes new species may come by a single step ; often, no doubt, by very few. But how much importance to give to the accumulation of small differences in the origin of species is not known. In the case of the giraffe, as with many other forms, the species from which it came has no doubt died out ; we can only speculate about its exact origin.

Before concluding, there is one aspect of the giraffe's evolution which must be mentioned. The giraffe, besides its long neck, is characterized like the flamingo and other birds by long legs and a body of suitable proportions ; at first sight these characters seem independent, suggesting that the new form must have evolved gradually enough to allow the production of individuals properly co-ordinated in their separate parts. Actually, the different characters are not independent. The form of an animal depends upon the relative rates of growth along different axes or in different parts. The giraffe owes its shape primarily to an increased growth rate along a vertical axis ; and it is clear that in this way a profound change in shape might occur as the result of a single mutation, without disturbing harmony of form or the co-ordination of different organs.

Genetics can therefore give a definite answer to some questions about the mechanism of evolution ; but none, so far, to others. It demands the origin of new forms by mutation, and the selection of those that are fittest to survive. It has had some success in analysing the different kinds of variation, especially those with a cytological basis. In some cases it has shown why it is that living organisms often occur in groups which make classification and naming possible ; and there is every indication that progress in solving this particular problem will be rapid. But it is not known how

far this cytological variation could have been responsible for progressive evolution, and it is doubtful whether factor mutations of the kind needed to explain evolution have yet been observed.

It is interesting that when Darwin put forward his theory of evolution he called his book *The Origin of Species*. In his day, though it was admitted that there could be variation within a species, it was thought that the bounds of this variation were fixed, so that species were immutable. By showing how new species could come, Darwin believed he had shown that evolution was possible.

Today, the position is curiously different. The origin of species by polyploidy has been experimentally demonstrated, and something has been learned about other ways in which species may arise. But these appear to be special kinds of variation, and it is uncertain how much they will account for evolution.

On the whole, however, it is the progress that has been made by genetics that is remarkable. A very precise body of knowledge has been built up, resting on three fundamental principles—the existence of constant hereditary units, the location of these units in the chromosomes in linear order and the pairing of homologous particles at meiosis—the whole giving a coherent and logical structure that cannot be found elsewhere in biology. It is unlikely that these principles will be disproved, though they may in time be superseded by still wider generalizations. A fresh principle that would alter our outlook on evolution may still be discovered ; but this could not overthrow what has already been done.

Much about the mechanism of evolution is still imperfectly understood ; but much has been learned. And whether the existing principles and methods of genetics should solve the problem or not, they will go far in that direction.

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